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LIQUID CRYSTALS.

THE very name seems to be a self-contradiction. How can a liquid be a crystal, and how can a crystal be a liquid?

The term was introduced by Dr. Lehmann, of Karlsruhe, and relates to certain remarkable bodies which, if we accept his interpretation, must entirely revolutionise current views regarding the structure of solids and liquids.

In previous articles I have attempted to explain the newer theories of crystal structure, and have suggested that in their geometrical aspect they harmonise in a most satisfactory manner with observed facts. Now the very essence of these theories consists in the solidity of the crystal structure; it all the more behoves me, therefore, to call attention to the work of an investigator who regards these theories as of no essential importance and would deprive them of the basis on which they have been erected.

The observations of Lehmann are so curious and surprising that, whether his interpretation of the facts be correct or no, both the facts and the interpretation deserve serious attention. Although nearly six years have passed since the publication of his papers on the subject, little more than brief mention of them, so far as I am aware, has appeared in English; they have not been repeated either to be verified or invalidated; authors who have referred to them have been careful to quote the observer's own words, without either accepting or rejecting his conclusions; or, if they have expressed any criticism, have done so in an

extremely guarded manner; and rightly so, for Lehmann is an acute observer and experimentalist who has contributed innumerable discoveries in the region of microscopical crystallography, and it was during the study of certain crystalline preparations by the microscopical methods of which he is a master that he encountered the remarkable phenomena which are the subject of the present article.

It was a botanist who, as in the case of so many interesting physico-chemical discoveries, contributed the initial observations. Dr. Reinitzer, of Prag, found that Benzoate of Cholesteryl had *two* melting-points, melting first at 145° to a dull and then at 178° to a clear liquid; also (more curious still), that the former appeared to possess a certain double refraction. He forwarded the substance to Lehmann for microscopical study.

Shortly afterwards, Dr. L. Gattermann prepared samples of three other compounds—Azoxyphenol, Azoxyanisol and a substance having the composition $N_2 O_3 (C_6 H_4)_2 CH_3 C_2 H_5$—which behaved in a somewhat similar manner.

The crystals of Azoxyphenol, which we may take as illustrating the behaviour of all the three latter compounds, exhibit the following peculiarities. Warmed on a microscope slide they are suddenly transformed, at a temperature of 134°, into a substance which preserves the outline of the crystal, is strongly doubly-refractive, becomes dark four times when rotated on the microscope stage between crossed Nicols, and therefore behaves in all these respects like a true crystal. As is well known to all students of the subject, crystals differ from other substances in being anisotropic while they are homogeneous; that is to say, all the properties of a crystal, while the same along parallel lines within it, are in general different in different directions. In the matter of their optical properties this character expresses itself in the double refraction exhibited by all crystals save those which belong to the cubic system; and as a result of this birefringence, if the crystal be placed between two polarising Nicol prisms whose principal planes are at right angles, light is in general transmitted through the combination and is only extinguished four times as

the crystal is rotated through 360° on the microscope stage.

Azoxyphenol at 134°, therefore, behaves in these respects like a crystal, but, incredible as it may seem, is nevertheless a liquid; it does not retain a geometrical form, but is free to move in all directions.

If the preparation be still further warmed, it passes at 165° into a third modification which is also liquid, but not doubly refractive. It is possible to contrive that this molten substance shall contain small portions of the first, birefringent, liquid which float about in it as perfectly spherical drops.

Crystals of Azoxyanisol and the remaining substance resemble Azoxyphenol in their behaviour, the changes taking place at 116° and 134° with the former, and at 87° and 140° with the latter. The drops are regarded by Lehmann as really liquid crystals. It must be confessed that from his account of their appearance their optical characters do not seem to be precisely those of ordinary crystals, although, as we shall see, they do somewhat resemble those of warped or deformed crystals; the directions of extinction of one of the drops viewed through the crossed Nicol prisms constitute a series of radial and concentric lines corresponding in form to the equipotential curves in a spherical conductor which is traversed by an electric current entering at one end of a diameter and leaving at the other end. Viewed along the diameter, the drops present a black cross resembling that produced by sphæro-crystals or by material having a spherulitic structure.

Further, when viewed through one Nicol prism alone they are seen to be dichroic and to present different colours in adjacent quadrants. We have to do then at any rate with doubly refractive drops which possess a symmetrical disposition of their peculiar optical properties, and are unlike any other drops or any other liquid previously described.

In recent years many crystals apparently belonging to the cubic system have been found to be birefringent, and long and fierce has been the strife between those who have

offered explanations of their so-called optical anomalies. Some of these crystals when examined in thin sections in the polarising microscope are seen to possess a weak birefringence and to be partitioned into symmetrical areas, quadrants or sextants for example, in which the light may be extinguished uniformly over alternate areas successively as the crystal is rotated between the crossed Nicol prisms, and not over the whole section as in an ordinary crystal. Alum, Garnet and Boracite are familiar examples.

Other apparently cubic crystals which exhibit optical anomalies present the regular features of uniaxial and biaxial crystals belonging to other systems, the orientation of the optic axis, or of the optic axes, being different in the adjacent symmetrical partitions.

Neumann, Klocke, Klein and others have attributed the birefringence to strains set up either during crystallisation or during fall of temperature and resulting contraction after they have solidified; Mallard on the other hand propounded the theory that such crystals are pseudosymmetrical and consist of a number of uniaxial or biaxial individuals possessing less than cubic symmetry, twinned together so as to simulate the symmetry of the cube.

Both processes are illustrated by certain well-known examples. There are without doubt crystals belonging to the orthorhombic or monosymmetric systems which do aggregate themselves by twinning and constitute a complex crystal of apparently higher symmetry than their own, and it is equally true that birefringence may be produced by pressure both in cubic crystals and in uncrystalline substances; it is even possible to make gelatine moulds in the form of crystals, or of other geometrical solids, which perfectly simulate the optical characters of a uniaxial or biaxial crystal, and when examined in convergent polarised light show the familiar coloured circles or lemniscates characteristic of a crystal.

Lehmann, however, dismisses the possibility of his liquid crystals being merely very soft solid bodies, whose birefringence is produced by one or other of these causes or by the inclusion of some impurity, on the following

grounds: one of his drops suspended in a liquid of a density equal to its own assumes the form of a perfect sphere, like Plateau's drops of oil in a mixture of alcohol and water, and this would be impossible if the action of the surface tension called into play any opposing elastic force. He concludes that the particles of the drops are free to move as in a true liquid.

In the course of his numerous microscopic observations Lehmann has adduced many examples of crystals which have been mechanically deformed and continue to grow with bent faces and curved outline. He argues that a crystal does not cease to be one when it is bent; and that the extent to which it will recover its original form when the tension is removed depends merely upon the elasticity of the substance and not upon any essential difference between crystallised and uncrystallised material.

"According to current views," he says, "the crystalline condition consists herein, that the molecules are arranged in a regular order in which they are retained more or less rigidly by the elastic forces. Under certain conditions the system can be disturbed by heat, pressure, etc.; it then passes suddenly by virtue of local pressure into a new system which is more stable under the altered conditions, just as a half-opened pocket knife closes with a snap." Against this view he enters a forcible protest.

It is true that many crystals do behave in this manner; the researches of Mallard and of Lehmann himself have disclosed numerous instances in which a crystal when heated passes suddenly from one solid modification to another without any transition through the liquid condition. Nothing can be more startling than the behaviour of such crystals when seen for the first time.

A section of Boracite viewed in the polarising microscope is seen to be traversed by a number of doubly refracting lamellæ; but when the section is warmed to a temperature of 265° a cloud seems to pass suddenly over the crystal, which becomes quite dark; as the section cools twin lamellæ suddenly make their appearance again at the same temperature.

Again, to take another example, Pope has recently described the behaviour of Chloral Hydrate which, when melted on a microscope slide and allowed to cool, solidifies as a thin crystalline film consisting of needles of a uniaxial substance; if this be allowed to stand, biaxial plates begin to make their appearance among the needles and continue to grow until the latter entirely disappear, the uniaxial being entirely converted into the biaxial modification.

But between such substances and the liquid drops of Gattermann's preparations the difference is great. Something like a transition between the two is supplied by the Benzoate of Cholesteryl discovered by Reinitzer and studied by Lehmann in 1889, shortly before the other three yet more remarkable compounds came under his notice.

When the benzoate is warmed to 178·5° it melts to a clear liquid, and if this be allowed to cool slowly it becomes transformed into a dull white aggregate; at 145° true crystals make their appearance in this and continue to grow rapidly at the expense of the white mass, but when warmed they are again converted into the white substance. The granules of which the latter consists can be conveniently examined if the temperature be maintained at 178°, and though not absolutely liquid are found to be so plastic that they can be moulded and made to run into each other.

Lehmann's interpretation of these bodies was called in question by Quincke (6), who suggested that their behaviour was due to the presence of oil; but Lehmann has repeated the experiments with extra precautions and has further described the still more remarkable features of soft crystals of potassium oleate, which orientate themselves, come together in parallel positions and unite to form a single crystal.

Ordinary crystals are solids which can be deformed up to a certain point without any permanent change being produced in their form; they may, for instance, be bent by pressure and then return to their original form when the pressure is removed; unless the limit of elasticity has been passed, when they are permanently deformed or broken.

Now Lehmann has found that in many crystals the limit of elasticity is so low that without actual disruption they may by the application of very slight force be made to flow like shellac or certain other amorphous substances. He has long expressed his opinion that the essential feature of a crystalline structure is not, as is commonly supposed, the regular arrangement of particles which are held together by elastic forces to form a more or less rigid structure. If this were the case, he argues, a sufficient deformation would destroy the structure and reduce the crystal to the amorphous condition, and this has never yet been effected.

He inquires, therefore, whether the limit of elasticity may not in certain crystals be not only extremely small, as in the soft substances previously described by him, but actually zero, so that the material may be liquid and yet crystalline. If a liquid exhibits polarisation phenomena similar to those of a deformed crystal, is there any reason, he asks, why we should not regard it as a liquid crystal? In a bent crystal the optic axes and the directions of extinction are no longer straight lines but curves, and the birefringence is possibly to be attributed not to the relative arrangement of the particles but to their anisotropic nature. According to this view, the fusion of a crystal is simply its conversion into a fresh crystalline modification in which the limit of elasticity is zero, so that the substance behaves as a liquid; if the molten crystal is devoid of birefringence it may be that it represents a modification belonging to the cubic system.

It is suggested that the soft crystals prepared by Dr. Reinitzer and the liquid globules of Dr. Gattermann's compounds are crystalline liquids which possess a very strong double refraction. Reference must be made to the original memoir of Lehmann for a full account of the remarkable manner in which the liquid drops behave. His figures represent the normal drops viewed in the polarising microscope, when each resembles a globe with its meridians of longitude and its parallels of latitude marked upon it; the axis appears to resemble in some degree the optic axis of a uniaxial crystal. The spheres might be sup-

posed to consist of birefringent uniaxial particles arranged with their axes along the meridians; during its formation, since any elastic force which might act in opposition to the surface tension is lacking, the particles would flow from one part to another in such a way as to make the surface potential energy a minimum, and the crystal would, like a drop of amorphous liquid matter, assume the form of a sphere. In the ordinary solid crystal the surface tension is different for different faces, and the crystal assumes a polyhedral form. The figures which accompany the memoir also illustrate the curious manner in which the drops may be deformed and broken up by squashing them between the slide and a cover glass. A globule may be broken up into two identical globules in this way; two distinct globules may even be brought into contact and merged together so as to constitute a single globule.

The question next arises whether the isotropic liquid into which the liquid crystals are converted by further heating is also a crystalline liquid belonging to the cubic system, in which case the transformation of the drops to the isotropic liquid is merely a passage of a liquid from one crystalline modification to another, analogous to the passage of the birefringent lamellæ of boracite to the solid cubic modification when a crystal is heated to 265°. Now it is found that when boracite and similar substances are transformed into allotropic modifications the new crystals are regularly orientated with regard to the old. In the same way when the liquid crystals are transformed into the isotropic liquid by rise of temperature, and back again to the birefringent drops by fall of temperature, the latter preserve their original orientation. It is as though the terrestrial globes which they resemble had been fused into a liquid mass, and on separating again from this on cooling, solidify with their axes pointing the same way as before.

Hence, it is argued, the isotropic liquid may be only a new crystalline modification of the liquid crystals, and that, just as is the case in the transformation of solid crystals, the one modification may be converted into the other

without loss of orientation in the particles. In other words, in answer to the question with which we started, it is suggested both that crystals may be liquid, and that liquids may be crystalline.

It is scarcely necessary here to follow the author through the remainder of his inquiry; let it be sufficient to mention that the three sorts of liquid crystals behave like isomorphous substances and mix together in all proportions; that they always separate as pure drops from the liquid into which they can be converted by heating, and do not possess the power of dissolving substances which are soluble in the latter.

It will now be clear that Lehmann's definition of a crystal is no longer the current one. The regular arrangement of its particles into a homogeneous anisotropic solid is not an essential but an accident which may occur under certain favourable conditions.

The essential characteristic of a crystal is, according to his view, the power of growing in a supersaturated solution which is possessed by all crystals but not by colloids. Some liquids also may be crystalline, others may be colloids, the majority are probably the latter.

If I may venture on a word of criticism I would urge that here we are concerned partly with a question of words. It will be wise to retain the names crystal and crystalline in their old significations, rather than to extend them so as to include the birefringent liquids whose existence has been established by Lehmann. It may be that these remarkable drops are examples of liquid matter in which particles while free to move are compelled to preserve the same orientation, and differ in this respect from ordinary liquids. But whether this peculiarity of structure, whatever may be its nature, is really analogous to that of solid crystals is a question in which it will be better not to commit ourselves to an answer by applying the same name to both until more is known about the structure both of liquids and solids.

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SUGAR: THE OUTLOOK IN THE COLONIES.

PERHAPS one of the most striking phenomena connected with the food of nations is the rapid increase in recent years of the consumption of sugar.

Between seven and eight million tons are used in various ways every year. When it is remembered that sugar is not usually regarded as a necessary food, but is rather classed as a "spice" to render other food palatable; and, further, that we have no means of judging the vast quantities used in China and India, and have accordingly not included them in our estimate, these figures appear to be well worthy of note.

The proportional quantities, again, in which this vast mass is shared by the different nations is a matter of considerable interest. The English-speaking races are the great sugar eaters. The average consumption of Great Britain and Ireland is 86·09 lbs. of sugar per head per annum; in the United States the proportion is 62·6 lbs. per head, while in France and Germany the figures are 30·61 and 26·78 respectively. This is all the more surprising when we consider that Germany at the present moment produces nearly double the amount of any other country, while France is an acknowledged rival in export.

This apparent anomaly is due to the curiously tangled state of affairs brought about by the "Kriegsprämien," the "war bounties". In France and Germany sugar is still a luxury—a spice—whereas it is becoming daily a more necessary article of Anglo-Saxon food.

The present condition of the sugar industry is thus peculiarly a matter of British interest. And, when it is broadly stated that most of the British Colonies engaged in growing the sugar-cane appear to be on the verge of bankruptcy, the interest becomes acute. The depressed condition in these Colonies may be traced to various causes, chief among which are (1) the competition of

European-grown beet, and (2) the various diseases at present attacking the canes. In the present paper it is proposed to deal more especially with the first of these.

Let us consider, at the outset, our national position regarding sugar. The British Isles consumed, in the year 1700, 10,000 tons of sugar; this amount had increased in 1800 to 150,000 tons, while the present consumption is 1,566,000 tons. Why has sugar taken such a hold on the people's fancy?

In Germany and France the quantity used is restricted by taxation, but in Britain sugar has a free market. This, in the main, explains the preponderance of British consumption. All races eat sugar if they can get it; and it is becoming more certain every day that this preference is due to the fact that sugar is a valuable muscle-making food.

It is perhaps worth while to dwell somewhat upon this point, for, although a matter of common enough observation in the Tropics, the facts are perhaps hardly appreciated among Europeans.

A striking example of the effect produced by sugar upon the human system may be seen among the labourers on West Indian sugar estates. There is a very evident improvement in the appearance and health, and, above all, the muscular strength of the negroes as the reaping of the crop proceeds. During this time they simply *live* upon the sugar-cane and its expressed juices. A doctor named Moseley, writing in the old slave days, noticed the same thing, and thus states the case in language more forcible than elegant:—

"In the West Indies the negro children, from crude vegetable diet, are much afflicted with worms. In crop time when the canes are ripe these children are always sucking them. Give a negro infant a piece of sugar-cane to suck and the impoverished milk of his mother is tasteless to him. This salubrious luxury soon changes his appearance. Worms are discharged, his enlarged belly and joints diminish, his emaciated limbs increase, and if the canes were always ripe he would never be diseased. I

have often seen old, scabby, wasted negroes crawl from the hot-houses apparently half dead in crop time, and by sucking canes all day long they would soon become strong, fat and sleeky."[1]

Edwards in his history of the West Indies observes that "the time of crop in the sugar islands is the season of gladness and festivity to man and beast. So palatable and salutary is the juice of the cane that every individual of the animal creation drinking freely of it derives health and vigour from its use. The meagre and sickly among the negroes exhibit a surprising alteration in a few weeks after the mill is set in action. The labouring horses, oxen and mules, though almost constantly at work during this season, yet, when indulged with plenty of green tops of this noble plant, and some of the scummings from the boiling house, improve more than at any period of the year; even the pigs and the poultry fatten on the refuse."[2]

It may be objected that the raw juice of the sugar-cane is very different from prepared sugar, and this is perfectly correct. While the sugar-cane may be advantageously eaten as a fruit, quantities of raw sugar are not likely to prove so wholesome. On the other hand it has been demonstrated that, at the moment of muscular action, grape sugar is fed to the muscle cells by the blood and oxidised.[2]

A practical test of the value of sugar as a muscle maker has lately been supplied by Dr. Vaughan Harley, who gives his results in a paper read before the Royal Society.[3] Dr. Harley's experiments were conducted upon himself with Mosso's "ergograph," and great care seems to have been taken to obtain accurate results. He first of all determined the value of sugar when taken alone in the production of muscular work, and, on a fasting day, increased his capacity for work by 61 to 76 per cent. He next proved that the addition of sugar to normal diet had a marked effect in retarding fatigue. Seven ounces of

[1] Lunan. [2] Chauveau and Kaufmann. [3] Vaughan Harley.

sugar added to a small meal, increased the work done by his right and left hands 39 and 6 per cent. respectively, while after a heavy meal increments of 8 per cent. and 16 per cent. were registered. Lastly, by taking 250 grams (8¾ oz.) of sugar in addition to the ordinary diet, the work of an eight hours' day was increased 22 to 36 per cent.

We have no reason then to be ashamed of our national liking for sugar. The outcry regarding its injuriousness to the teeth is not often heard now; and one is tempted, with old Lunan, to suggest that the marked preference of children for sweet things may very probably be one of those curious instincts which lead animals to choose their proper food.

It is interesting in view of these facts, to speculate upon the connection between our national sugar bill and the British taste for sports and violent exertion, so little relished or almost unknown among continental nations. We read that the American Indians, when making a difficult journey, used to provide themselves with a mixture of maple sugar and Indian corn, in the form of pressed cakes, half meal, half sugar. The well-known antiseptic properties of the sugar kept the food fresh, and a few spoonfuls of the mixture in half a pint of water formed a "pleasant and strengthening meal".

The following plants produce the sugar of commerce—the sugar-cane (*Saccharum officinarum*), the beetroot (a variety of *Beta maritima*), the sugar maple (*Acer saccharinum*) and certain tropical palms. Of these the two former are vastly more important and will alone be considered.

The division of commercial sugar into that obtained from the beetroot and that produced by the sugar-cane is a natural one. The two plants belong to very different orders, and the sugar is contained in different regions of the plant body. The processes by which the sugar is extracted are frequently dissimilar. The plants are denizens respectively of temperate and tropical climates, and are therefore produced under widely divergent conditions. It

thus becomes an easy matter to state the proportional quantities derived from these two sources for the world's market.

The following appears to be the approximate yield of sugar during the last two years, leaving China and India out of consideration as countries which do not export and concerning whose home consumption we have no data :—

	1894-5.	1895-6.
Beet, - -	4,792,530 tons	4,323,899 tons
Cane, - -	3,387,461 tons	2,652,000 tons
Total, - -	8,179,991 tons	6,975,899 tons

(The Cuban War, together with a diminished beet crop, accounted for the fall of over 1,000,000 tons in yield. The consumption was normal.)

It is clear that the world's market is already in the hands of the European beet producers; and it is most probable that during the next year or two the production of beet will be considerably increased, while there seems to be small hope of the extension of cane-growing because of the maimed condition of Cuba, the principal contributor.

The beetroot as a sugar-producing plant has a comparatively recent history, whereas the sugar-cane has been cultivated from time immemorial. The rapid increase of the beet industry may be gauged by the following facts. In 1747 Andreas Sigismund Marggraf, a professor in Berlin, made the important discovery that the sugars in beetroots, carrots and other fleshy roots of temperate countries were chemically identical with one another and with that of the sugar-cane; the sugar of ripe fruits, such as the grape, and that in honey being placed in a different class. This discovery did not however bear immediate fruit, and the first beet factory was not erected till 1801, near Breslau, in Silesia.

From the imperfect nature of the processes of extraction, and the great difficulty in eliminating the impurities from beet juice, it is more than likely that the enterprise would have died in its infancy. At this point, however, as has so

frequently been the case with beet sugar, a powerful impulse was received from the political condition of Europe. There appears to be no doubt that the introduction of beet factories into France by Napoleon I. was intended as a blow to the British Empire through her Colonies. England had attained her greatness through her Colonies, and Napoleon was determined to destroy them. The Continental ports were therefore closed to free British sugar; the price rose rapidly, and, under this artificial political protection, factories sprang up in various parts of France and Germany. Most of these collapsed, however, at the downfall of the Emperor.

We do not again hear much of beet-growing till about 1840. The British West Indies had just received their first serious rebuff in the emancipation of slaves, and it is quite probable that by this means the cultivation of the beet again received a political stimulus. From this time onward, the manufacture of sugar in Europe steadily and rapidly increased. The advance was and is artificial, in that payments are made on the Continent either directly or indirectly on all exported sugar; on the other hand, the British market, formerly a Colonial monopoly, has been opened more and more widely to the world's supply, first by the abolition of differential duties in favour of the Colonies, and later by the placing of sugar on the free list.

The British sugar-growing Colonies, being denied the protection they formerly received, have therefore steadily lost ground. The imports of sugar into the United Kingdom have increased five times during the last fifty years, whilst that received from the Colonies has diminished. This is partly due, of course, to their finding other nearer markets (the United States for West Indian Colonies and India for Mauritius), but the total production of the British possessions will not at present meet the home demand.

The accompanying table will illustrate the growth of British imports during the last fifty years, the gradual decrease of sugar received from the Colonies and its almost entire replacement by European beet:—

	1845.	1865.	1895.
Total Imports,	6,000,000 cwts.	11,000,000 cwts.	31,000,000 cwts.
From British Colonies (Cane),	5,000,000 cwts.	5,000,000 cwts.	3,000,000 cwts.
From Europe (Beet),	4,000 cwts.	300,000 cwts.	23,000,000 cwts.
From Germany alone,	———	30,000 cwts.	17,000,000 cwts.

Great Britain offers the only free market in the world. The great sugar-producing countries of Europe offer bounties on production or export, but find the funds for this by taxing the home consumption. An enormous agricultural and manufacturing industry is kept going by these at the cost of an enormous national burden. This burden is fast becoming unendurable, while, on the other hand, the privileged class of bounty-fed agriculturists is becoming more powerful a factor in the State. The net result for Great Britain is cheap and plentiful home sugar and the collapse of the sugar Colonies.

It is obvious that all this has an important bearing on the fact already alluded to, that we, in these small islands, are enabled to appropriate, as we do, nearly half the European consumption and nearly a quarter of the world's supply. In the agitation now on foot for the salvation of the sugar Colonies, the economic soundness of a protectionist policy is much to the fore.[1] The question is complex and many-sided, but one of the best rejoinders which free traders can make is a reference to the past effect of taxation upon consumption.

From 1800 to 1845 the British demand for sugar hardly kept pace with the increase of the population. The average consumption during this period was about 18 lbs. per head per annum, but in 1845 it had sunk to 17 lbs. At this latter date the import duty was reduced, and in four years the consumption increased nearly 50 per cent. "The

[1] *St. James's Gazette.*

protectionists maintained that the reduction of the duties would bring about no perceptible increase in the consumption of sugar. Lord George Bentinck, the champion of the sugar monopolists, went to the length of showing that under no conceivable circumstances could the people consume more than 5,600,000 cwts. of sugar in the course of a year. In the third year after the prophecy was issued the total consumption rose to considerably over that figure."[1] In 1854 the consumption had exactly doubled its former figure, reaching 34 lbs. per head per annum. In this year the duties were increased again because of the Crimean War, and the consumption dropped from 34 to 28 lbs. From the close of the Russian war to the present time, finally, the consumption has steadily increased, reaching as already mentioned 86 lbs. *per capita* in 1895.

We have seen the effect of the Continental system of premiums upon the Colonial produce, and there seems little hope of improvement in the Colonies until the bounties are abolished. What likelihood is there of the latter taking place? The competing Continental nations are feeling the pressure of sugar taxation severely. Of these the principal producers are :—

Germany,	1894-5,	1,844,586 tons.
Austria,	,,	1,055,321 tons.
France,	,,	792,511 tons.
Russia,	,,	615,058 tons.
Belgium,	,,	243,957 tons.
Other Countries,	,,	240,597 tons.

Various attempts have been made to arrive at an international understanding with the object of abolishing the bounties, as it is felt that the position is abnormal in that the industry is no longer such a one as needs artificial fostering. Obviously such action must be agreed upon by the larger exporting countries—Germany, France, Austria, Belgium and Holland. At the London Conference (1887) the British Government took the initiative in this direction,

[1] Reed.

but without result. In 1895 Austria and Germany opened negotiations without success. France alone refused to do away with the sugar bounties.

On the 30th of May of the present year, finally, a new Act was promulgated by Germany on the bounty question, to last till 1903. This is avowedly a warlike Act to compel France to agree to the abolition of premiums. As at first drafted, these were to be raised from 1·25 to 4 mks. per 100 kilos. on raw sugar, and from 2 to 5·25 mks. on refined. The adopted rates are, however, 2·50 mks. on raw and 3·55 on refined sugar; and the question is naturally mooted as to whether the original intention is still maintained. Is it a warlike move to render the continuation of the bounty system impossible—the burden devolving upon the tax-payers is supposed to be much greater in France than in Germany, the yield is lower, labour is dearer and there are fewer people to bear the load—or is it yet another concession to the powerful agricultural class, making the towns pay for agricultural depression?

The result of this legislation it need hardly be said is being watched with breathless interest. Austria has immediately followed suit: has indeed made a larger increase in her bounties. Holland is concerting measures. All eyes are fixed on France. What will be the next move in this bitter bounty war?

It will be readily seen that, while the cane-producing countries of the tropics were formerly in straitened circumstances, the effect of this new legislation is disastrous—the price of sugar being, as far as gold countries are concerned, below the cost of production. The British Colonies may be ruled out, as far as export is concerned, and, when it is remembered that some of these Colonies have all their eggs in one basket—growing nothing but the sugar-cane from one end to the other—and, further, that in their long struggle they have either locked up all their available capital in costly machinery, or are heavily mortgaged, it becomes a question of imperial policy whether the powerful and wealthy mother country cannot with advantage lend a helping hand to her struggling depen-

dencies. Otherwise, unless the "war bounties" cease, inevitable ruin stares these Colonies in the face.

In the presence of this great economic problem, all other considerations are placed in the background. The future seems only too clearly defined, although there may be temporary relief because of the state of affairs in Cuba, by whose war close upon 1,000,000 tons a year are withdrawn from the markets of the world.

There are other circumstances handicapping the cane-growing countries for which the beet growers are not responsible. The machinery on many of the estates is of a very inferior character indeed, and the amount of money sunk in this old machinery makes it very difficult for the impoverished owners to replace it by new. The method of extraction is therefore very inferior, and stands little chance of improvement. This matter of methods has always been held in the forefront by beet growers, because of the much greater difficulty in obtaining a pure sugar from their plant than from the cane; and the successive stages by which the beet plant itself has been improved in its saccharine content, and the processes of extraction and purification have been perfected, are well worthy of study, forming, as they do, a signal triumph in applied botany and chemistry. Suffice it to say that while in 1836 eighteen parts of roots were necessary to produce one of sugar, this quantity was reduced to 13·8 parts in 1850, 12·7 in 1860 and 9·25 parts in 1887.

Lulled into a false security, the cane growers have been content meanwhile to retain the same machinery by which a large proportion of available sugar has been passed to the refuse heap; and now that more economical working is necessary, circumstances render it difficult for them to extricate themselves from their false position. The time appears to have arrived when bankruptcy will clear away these cumberers of the earth and make room for others more ready to take advantage of the lessons which science is teaching them.

The fruits of adversity are bitter, but salutary, and great strides have undoubtedly been made in many places in the improvement of machinery as the successive blows have fallen upon the Colonies—first, the abolition of slavery, then the abolition of differential duties, and with these the monopoly of the British market, and lastly, and more crushing still, the unfair competition of bounty-fed beet.

If these bounties are abolished, the future of the sugar industry will doubtless be as full of surprises as the past has been. A renewed and more extended conflict will take place before cane and beet find their respective markets. It may be safely surmised that in Europe the removal of restrictions upon home consumption would vastly increase the quantity required—it would probably be largely increased in France, and is even now steadily increasing in Germany. In Britain numerous industries would be affected, prominent among which would be the decayed refining industry and the greatly developed jam trade. And, sobered by sad experience, the Colonies would take up the question so long neglected of bettering their extracting processes, and new machinery on economical lines would lay the foundation to a future of profitable industry.

But the planters would also have to devote increased attention to the improvement of their canes—not only as regards their saccharine content, but also with regard to their capacity for resisting disease. There are, in fact, at the present moment, diseases in the cane fields which threaten to sweep away some of the most magnificent varieties of the sugar-cane.

There are many scientific questions of great interest connected with the origin and spread of these diseases, and it is proposed in a future paper to deal with these, and pass under review the strenuous efforts being made by scientific men in the tropics both to produce improved varieties of the sugar-cane and to combat their diseases.

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THE CELL AND SOME OF ITS CONSTITUENT STRUCTURES.[1]

FROM old time it has been one of the highest aims of Philosophy to penetrate the veil which obscures the mysteries of life and of organisation, and the concentration of such an immense amount of attention on the minute details of cell structure during the past few years is the outcome of a conviction that this is one of the most favourable means of attacking the problems we seek to solve. But so vast a mass of knowledge has been acquired that perhaps we run some danger of missing the goal amid the maze of paths which have been opened up towards it. And thus it may not be altogether amiss to stand still for awhile and try to take stock of our position, to criticise the various conflicting opinions, and to test as far as may be, the foundations on which they rest. And one of the first facts to which it is necessary to be warily alive, in inquiries such as these, is that the vast majority of the observations on which we rely have been made on the dead cell. That is, we are trying to form a conception of what goes on in *living protoplasm* from results gained by a study of structures preserved in a dead substance which is certainly no longer protoplasm. We assume, but we ought to be constantly testing the validity of our assumption, that in what we call well-fixed material, at any rate the grosser structural arrangements of the protoplasm as it existed in the living condition are preserved to us. But the difficulty lies exactly in determining our criterion of proper fixation, and a neglect of this precaution may render utterly valueless even an accurate description of what an observer sees, unless he can satisfy his readers that he is not, as a matter of fact, merely describing artefacts, appearances artificially produced during the process of killing. Indeed, one often hears this *a priori* objection urged against cytological work

[1] Read before a combined meeting of Sections D and K of the British Association, 22nd September, 1896.

in general, and I think I may safely say that no one who has had much experience in actually watching the effect of reagents on the living cell will be inclined to under-estimate its importance.

I am not now speaking of those obvious contractions and distortions, the well-known results of defective treatment of the material at some stage between killing the protoplasm and finally observing its structure with the aid of the microscope. I am thinking rather of the insidious effects of certain reagents which, in some cells and tissues at any rate, are able to evoke *post-mortem* changes so definite in their character that only careful and wide comparison enables one to set them down as undoubtedly artificial. It may be the reagent was too much, or too little, concentrated, or that it happened to be unsuitable for some reason or another to the particular tissues under investigation.

But there are other perils, of a more general, and perhaps one may say of a more subjective, nature, to which the student of cytology is specially liable. I refer in particular to that one of over-generalisation. The fact is, the subject is so essentially a study of an enormous number of minute details, the relative importance of which is still for the most part uncertain, that one is more than usually exposed to the danger of drawing too wide conclusions from too narrow an area of fact. That this is a real and not a fanciful charge, a slight acquaintance with the first-hand literature of the subject is enough to prove ; elaborate general theories are based on the study of, it may be, a particular set of nuclei belonging to a single organism. The time so spent would have been better employed in extending the authors' range of vision over other groups of the animal and vegetable kingdom.

The phenomena which are successively exhibited during the processes resulting in the formation of the reproductive cells in *Ascaris megalocephala*, for example, and in their fusion during the act of fertilisation, are so striking that it is small wonder that they have been, and by some people seem still to be, regarded as representing a general type to which other organisms ought to conform, at least in the

broader outlines. Nevertheless, we know, as a matter of fact, that Ascaris will *not* serve as a general type; we know that differences exist between it and many other organisms in almost every detail, and that only the final result is the same. It is obvious, then, that clearness of differentiation alone will not serve as a criterion of fundamental importance in the case of any given structure, and, perhaps, put in this bald way nobody would expect it would. I have mentioned Ascaris, because it has come to be a familiar object to many people—all the principal phenomena of its nuclear division and of fertilisation can readily be seen without employing any of the more refined methods of technique. But I might equally well have taken the Salamander, which has served as the chief object of Flemming's epoch-making researches, and I may the more specially mention the Salamander in this place, since it differs in so very many details from Ascaris.

Having cleared the way thus far, I now propose to consider some of what we may perhaps regard as the chief groups of phenomena associated with nuclear division which are known at the present time. In this way I believe we shall be best able not only to estimate the relative importance of the several structures involved, and to understand their mutual interaction during the process of karyokinesis, but we shall further have acquired a material basis from which to criticise the views now current as to the way in which the fact of heredity is to be harmonised with the complex processes proceeding within the cells themselves.

For it is to the nucleus and the protoplasm that we must turn for the essential factors which determine the structure and govern the organisation of living beings. But we meet with differences of opinion at the very threshold of our inquiry, as to the share allotted to the nucleus and to the cytoplasm respectively in initiating the process. Some say the stimulus comes from the protoplasm, some say it comes from the nucleus, a third party ascribes it to that veritable *Deux ex machina* the centrosome; and Boveri, if I understand him rightly, postulates yet another organ, a specialised mass of protoplasm in the vicinity of the centrosome, which

he calls the archoplasm. Of all the so-called organs of the cell, none has been more vigorously investigated with so little agreement in general result as the centrosome, or centrosphere. In fact, when one dips into the bulky literature which has grown up around this body, the dimensions of which sometimes border on, and often dip below, the visible, one is filled with admiration at the vastness of the labour, and with sorrow at the utterly incommensurate result. Perhaps the great interest attaching to it is to be largely attributed to the clearness with which it presented itself to Van Beneden in Ascaris,[1] in which animal its importance was first insisted on. In many other animal cells it is also clearly seen, and I may say that in another invertebrate animal I have, thanks to the kindness of Professor Rückert in Munich, seen centrosomes almost surpassing those of Ascaris itself. But, on the other hand, there are many other animals in which these structures appear under a simpler form. The dense aggregation of protoplasm is not observed in a number of cases even at the time most favourable for distinguishing it if present. Again, even in Ascaris, it would seem that it is not exactly a permanent *morphological* constituent of the cell; for Boveri and others had observed that it becomes indistinguishable at a later stage of nuclear division. Here, then, we must assume a "physiological" persistence, or use some other expression to save the individuality of the "cell organ". But is it really necessary to regard the centrosome as a universally essential structure at all? We know that, curiously enough, in the divisions which result in the cutting off of the polar bodies from the maturing ovum, it is very commonly and perhaps typically absent, and there is a large amount of positive evidence to show that with the single exception of Myzostoma, described by Wheeler,[2] and apparently well authenticated, it is entirely lacking from the

[1] E. van Beneden et Neyt, "Nouvelles recherches sur la fécondation et la division mitotique chez l'ascaride mégalocéphale," *Bull. Ac. Roy. de Belgique*, 1887.

[2] "The behaviour of the centrosomes in the fertilised egg of Myzostoma glabra," Leuckart, *Jour. of Morph.*, vol. x.

mature ovum. Indeed, so generally is this opinion shared, that Boveri, one of the staunchest upholders of the importance of the centrosome, regards its importation by the spermatozoid during fertilisation as one of the necessary conditions of segmentation. But it is difficult to reconcile this assumption with the results of an investigation carried on by R. Hertwig[1] on Echinoderm eggs.

This writer, in the first place, agrees with all the recent observers who have investigated these objects; that Fol was quite in error in attributing a centrosome to the egg. In fact, the whole history of the supposed fusion of centrosomes arising from the male and female cell respectively is one of the best object lessons I know of wherewith to enforce the remarks about methods which I prefaced to what I have to say to-day. But R. Hertwig discovered that, although the female cell contained no centrosome (that is, no "organ of division"), this circumstance did not prevent the nuclei of these unfertilised eggs, when they had been kept for a sufficiently long time, from starting through the first phases of karyokinesis; and he incidentally made the interesting discovery that certain stimulants hurried up the process, so that, although normally it only begins after the eggs have been extruded about two days, under the influence of very dilute solution of strychnine it commences in about one and a half hours after extrusion. It is true that for the most part the division of the nucleus was not completed, but that does not affect our present argument; the important point is that in the admitted absence of the so-called *division organ* the division itself could be initiated.

But Hertwig went further with his experiments. He found that by applying chloral to the spermatozoids and ova, before mixing them together, he was able to inhibit the *fusion* of the male and female nuclei, although the spermatozoid still retained the power of penetrating the egg sufficiently to become embedded within its protoplasm. When this occurred, spindles were formed, both in connection with the

[1] "Ueb. die Entwick, des unbefruchteten Secigeleies," *Festschr. f. Gegenbaur*, Bd. i., Leipzig, 1896.

male and female pronucleus, but both were completely independent of each other. This proved two things: first, that the male pronucleus was able to undergo division on its own account when within the egg protoplasm, as had already been shown in another way by Boveri; and, secondly, that the stimulus,—whether arising in the first instance from the male nucleus alone, or as the result of the interaction of it with the surrounding protoplasm,—could be transmitted so as to set the other, the female nucleus, dividing. And there is another fact which seems to indicate that the division is, really, the result of the *mutual interaction between the nucleus and the cytoplasm*, namely, that a spermatozoid of Echinus, when introduced either into a ripe egg or into a non-nucleated fragment of such an egg, at once begins to divide, but that if introduced into a similar egg, or part of an egg, which is not ripe, no change whatever results. All these facts, I think, militate against the great importance claimed for the centrosome as an actual organ of division, and go to show that the *nucleus and cytoplasm as a whole* are the principals concerned in the process.

There is a good deal of uncertainty surrounding the individuality of the centrosome in plants. It corresponds to the centrosome in its original sense as used in animals, but perhaps not to the modern extension of it which has been invented to embrace those highly inconvenient structures described as microcentra by Heidenhein[1] and others. Sometimes the centrosome is marked off from the surrounding radiations by a clear space, and in this case the resulting sphere is termed the centrosphere. Now, it is probably a significant fact that all the clearest demonstrations of centrospheres have hitherto been met with in the lower plants. In some Diatoms they are said to be so obvious as scarcely to require staining to render them visible. In *Sphacelaria*, originally described by Strasburger,[2] they are extremely

[1] M. Heidenhein, "D. Hautdrüsen, d. Amphibien," *Sitz. d. Würz. b., Phys. Med. Gesells.*, 1893; also, "Ueb. Bau. u. function d. Rriesenzellen im Knochenmark," *Würzburger Verhandl.*, 1894.

[2] Strasburger, *Schwarmsporen, Gameten, pfl. Spermat. u. d. Wesen d. Befrucht*, 1892.

clear during the actual stages of nuclear division. Whether they retain their identity during the actually resting condition seems to me to be doubtful. At any rate, it would require some confidence to discriminate them in the absence of all radiations, from the other granules which lie in the vicinity of the nucleus, each likewise surrounded by a clear sphere. In Liverworts again they are often extremely well defined, and at the same time they appear in such a peculiar fashion in many of these plants that I may perhaps be pardoned for drawing special attention to them here. It has not, so far, been found possible to detect with certainty the existence of centrosomes in the resting cells of these plants, but when nuclear division is about to take place, a minute but easily recognisable body is found at the spot from which the radiations are beginning to appear. During the successive bipartitions which go on in the young sporogenous tissue of such a liverwort as *e.g. Fossombronia*,[1] they are particularly plain at the earliest stages of karyokinesis. But when the spindle is complete and the chromosomes are forming the equatorial plate, the radiations die away at the poles and then a centrosome is no longer distinguishable. The same thing may be still more easily seen during the germination of the spores of *Pellia*, a very common Hepatic; and convenient, because the spores germinate while still within the sporogonium, and hence all stages may be found close together. But as the process of karyokinesis advances the radiations once more appear, and with them, the centrosphere usually again becomes visible. Is it possible that this temporary obliteration of the polar radiations is to be taken as the expression of a relatively stable, if transient, condition of equilibrium, and that, with the temporary cessation of movement in the protoplasm, the differentiation of the centrosphere and the attendant radiations are for the time being in abeyance?

Perhaps some one may object that, after all, I have as yet omitted to state the strongest evidence for the claims of

[1] Farmer, "Nuclear Division and Spore Formation in the Hepaticæ," *Annals of Botany*, vol. ix., 1895.

the centrosome to be regarded, generally speaking, as a permanent if often concealed "organ of the cell". I mean the wide-spread and apparently well-founded belief that it is not formed *de novo*, but only arises as the result of division of a pre-existing centrosome. I shall revert to this question later; but in this place will mention three sets of facts which are difficult to reconcile with the absolute continuity-of-existence theory. The first I take from the animal kingdom. Brauer[1] observed, whilst investigating the development of the parthenogenetic eggs of *Artemia salina*, that there was no centrosome associated with the first polar division, but that afterwards when the egg was ready to segment to form the embryo a centrosome became visible. Brauer, it is true, remarks that the centrosome might have existed in a modified or distributed state during the first karyokinesis; but this is merely a pious belief, and all more recent work, with the exception of Hacker's statements as regards Ascaris, seems to tell against it. Brauer himself only puts it forward apparently as a means of saving the reputation of his egg centrosome. The second and third sets of facts I shall draw from instances of which I have more special personal knowledge. In the liverworts, the two main subdivisions of Marchantiaceæ and Jungermanniaceæ are characterised by differences in the formation of their spores. The former, the Marchantiaceæ, do not for the most part call for special notice. With the exception of *Fegatella* and *Targionia* the divisions in their spore mother cells so far as is known coincide in their main features with those seen during the formation of dicotyledonous pollen grains. That is to say, the nucleus undergoes two perfectly normal bipartitions. The shape of the mother cells is commonly (with the exception of the two genera above mentioned) spherical. It is widely different in the Jungermanniaceæ.[2] Here the mother cell, as it grows prior to its final division, becomes a four-lobed body, and

[1] Brauer, "Zur Kenntnis d. parthenogenetisch sich entwickelnden Eies von Artemia salina," *Archiv für Mikr. Anat.*, Bd. xliii.

[2] Farmer, *loc. cit.*

the degree of the lobing varies in different species. The important fact for our purpose is that in all these four-lobed cells, at the commencement of division, *four*, and not two, centrospheres appear simultaneously, placed symmetrically at the periphery of the nucleus. Further, in those spore mother cells in which the lobing is not very prominent, the four centrospheres ultimately coalesce in pairs, so that a normal bipolar spindle is the result as soon as the first division is completed, the daughter nuclei begin to divide in a perfectly normal, that is, bipolar, fashion. But in the cases of extreme lobing, as in *Pallavicinia*,[1] there is no room for a symmetrical bipolar spindle, and the four poles persist throughout the entire division. This is modified, inasmuch as two divisions of the chromosomes occur without an intervening period of rest, and thus the four daughter nuclei are formed simultaneously; transitional stages, however, connect the two extremes. The general conclusion impressed on the mind of the observer is, that it is the *shape of the cell* which in some way determines the proximate character of the division.

Now, these facts seem to me to militate strongly against the permanent organ—and individuality—hypothesis of the centrosome. How can one imagine that the centrosome could in these cases become divided into four, and direct the protoplasm to transmit each quarter to its right place, without betraying any evidence of the process? For it is hardly conceivable, that in a large mass of material, all fixed at the right time, such stages, if present, could have escaped observation when they were being directly searched for. To my own mind a more natural explanation seems to be that the centrosome in this case originated, *de novo*, at the spots at which they were found, and that their singular mode of distribution is to be regarded as the result of the relative degree of independence associated with the four masses of protoplasm, an independence due primarily to the greater or less complete severance of the living contents of any one lobe from those of the rest. And, furthermore, the

[1] *Annals of Botany*, vol. viii., p. 47 *et seq.*

otherwise astonishing differences observed between nearly allied plants are, on this hypothesis, no longer surprising.

It seems to me that a consideration of an exceptional case like the one just quoted, is especially useful in enabling us to form a judgment as to how far the whole process of cell division may be the result of physical forces operating in the cell, or how far we are justified in invoking the aid of a special organ of division to rescue us from intellectual uncertainty. It must be remembered that, if we have recourse to the centrosome as the prime operator in these matters, we practically give up attempts at arriving at a solution of the difficulties. For we are no nearer to comprehending the phenomena of cell division than we should be as regards the origin of protoplasm itself if we were to assume that it was brought here in the first instance on an aerolite. On the other hand, if we accept the view of Heidenhein that the centrosome is a mere insertion point of the forces set free in the protoplasm, its nature becomes a secondary matter, and we know, as a matter of fact, that its position may vary, it may be intranuclear during the spindle formation, as in the *univalens* variety of *Ascaris megalocephala*, or, on the other hand, it may be extranuclear, as in the other (*bivalens*) variety of the same animal. These questions as to position, however, lose their importance if we take this view of its *rôle* in karyokinesis. Furthermore, the disparity of size between the two centrosomes belonging to opposite ends of a dividing nucleus, which was pointed out amongst others by Reinke,[1] no longer astonishes us, nor does the fact that here and there spindle fibres seem occasionally to mistake an isolated granule for a centrosome, judging at least from the manner in which they diverge towards it. Still once more, the facts noticed by Belajeff[2] and others, that during the division of the pollen mother cells of certain flowering plants the spindle fibres are frequently at first *not* directed to two points, but only later converge thither, become less unintelligible; the directions of the strains have,

[1] Reinke, "Zellstudien," Theil II., *Archiv f. Mikr. Anat.*, Bd. xlix.

[2] Belajeff, "Zur Kenntniss d. Karyokinese b. d. Pflanzen," *Flora*, Erg.-Bd., 1894.

so to speak, not yet become focussed. Finally, the general radiation round the entire nucleus, figured by Guignard[1] and others in the pollen mother cells of *Lilium*, no longer surprises us, although, on the assumption of the existence of persistent centrosomes, whose supposed business it is to manage these matters, the phenomenon strikes one as odd.

Perhaps one might even go farther, and ask if the facts really warrant us in thinking that the centrosome is more than an indication of the position at which the forces are in equilibrium; perhaps the centrosome is no more the *cause* of the forces than are the dead leaves and broken bits of wood in any way effective in producing an eddy, the centre of which they mark. The obvious objection that in many cells the centrosomes endure from one division to another by no means proves that they act, even here, as the directive agents in initiating and presiding over each mitosis. We know practically nothing as to the mechanical conditions of protoplasmic contractility; but it may perhaps be pointed out that it is not necessary to suppose that the particles which we may for the moment assume to be coerced to form a centrosome, should disperse when in a medium of approximately the same density as themselves, even when the conditions which caused them to become aggregated, in the first instance, are no longer operating; and, secondly, I may mention the fact that they are best seen in tissues in which cell division is active, and thus one scarcely need look for their dispersal during the short quiescent intervals. Finally, there is the microcentrum of Heidenhein, in which it would seem possible that such a scattering of the particles is actually taking place. The fact of their division in cases like that of the well-known Hermann's spindle[2] hardly helps the matter one way or another. It will be remembered that Hermann discovered that the spindle arose, in the Salamander, as a concomitant of the divarication of the centro-

[1] "Nouvelles Études sur la Fécondation," *An. Sci. Nat.* (*Bot.*), 7ᵉ Série, t. xiv., 1891.

[2] For figures and description see O. Hertwig, *The Cell* (Eng. trans.), p. 185.

somes quite independently of the nucleus. Whether we suppose that forces are at work in the protoplasm which split the original centrosome and drive the two halves asunder, with the resulting formation of the so-called central spindle, or whether the *vis viva* originates in the centrosome itself, the appearances would be precisely the same, and, as a matter of fact, both views have been urged.

But if we allow that the centrosome owes its existence to the energy liberated in the protoplasm, this does not exclude the hypothesis that a periodical repetition of certain conditions might as often lead to the formation of a centrosome. Indeed, from what we already know of protoplasm, it is not difficult to imagine, should the presence of such a structure conduce to the more adequate accomplishment of karyokinesis, that it might probably be differentiated in this way. All the facts we possess on the subject of nuclear division go to show the existence of an almost infinite amount of variety in the details of the process, and any possible physical condition which facilitated its completion, or which enabled its course to be run through more smoothly, would probably come to be more or less closely realised. And the very belief that centrosomes *are* in some way useful, is founded on the frequency with which they occur, and on their relation with the spindle.

But however this may be, we can hardly escape the conviction, even on the independent-organ hypothesis, that it is the protoplasm which, after all, carries its "division organs" into those positions which determine the direction of division, a direction that is often, perhaps always, so exactly in accordance with what mathematical theory demands that its plane can be predicted from the contour of the cell. And in many of the apparent exceptions to this rule, as for example in the eggs rich in yolk, to be found in many animals, and perhaps also, though less clearly, in the oospheres of *Pinus*, the difficulty vanishes as soon as one realises that the protoplasm is not homogeneous throughout the whole cell, but is obviously of a different texture in one part, as compared with the rest.

Taking leave of the centrosome for the present, we may briefly glance at the structure and mode of origin of the spindle. And here, too, we are confronted by an astonishing amount of diversity in detail, accompanied by a surprising degree of similarity in the final result. The most complete form of spindle is that known as Hermann's spindle, to which I have already alluded. Here the primary fibres, which form a central strand, arise in the protoplasm and have in the first instance no direct reference to the nucleus at all; but, by-and-by, from the poles of this central spindle radiations extend through the rest of the cytoplasm, and are directed in special abundance to the nucleus. The chromosomes which are now in process of formation are roped up as it were on to the central spindle, finally becoming symmetrically arrayed around it. During the division of the chromosomes and the divarication of their respective halves to opposite poles the movement seems to be effected by a contraction of the peripheral roping spindle fibres, the central spindle merely serving as a kind of railroad along which the chromosomes travel.

There are, however, other instances in which the same thing is effected, but the mechanism is less complete. In the spore mother cells of *Fegatella*[1] the spindle is of a remarkable form, probably owing in part to the peculiarly compressed shape of the cell. The spindle is not formed in the same way as in the Salamander, but it originates simultaneously at two points not quite 180 degrees apart. Thus the chromosomes are at first grouped in an angle of the spindle. The point which is of interest to us just now is the fact that at the earliest stages there seem to be no fibres which run continuously from pole to pole; this condition is, however, speedily reached by the fusion of a number of fibres which meet half way between the two poles.[2] Then

[1] Farmer, "Further Investigations on Spore Formation in *Fegatella conica*," *Annals of Botany*, vol. ix.

[2] Drüner (*Jenaische Zeitschr. f. Naturw.*, Bd. xxix.) asserts that in the Salamander the central spindle begins as the result of a *fusion* of radiations, such as occurs in *Fegatella*. This is contradicted by Meves (*Archiv f. Mikr. Anat.*, Bd. xlviii., p. 19), who upholds the view that they are continuous from the moment at which the two centrosomes begin to divaricate.

the poles move farther apart, though whether owing to a *stress* in the now continuous axile strand or whether owing to the *strain* caused by the contraction of the peripheral fibres which run out to the cell wall, it is impossible to say, It suffices for present purpose that here, too, as in the Salamander, the triangular character disappears, and the chromosomes are pulled to the axile strand, on which they proceed to undergo division in the usual way. In other plants there is not such an obvious formation of a central core of fibres, but in reality it is probably of common occurrence, and a form of it can with certainty be distinguished in many cells; that is to say, some fibres may be seen to run right across from pole to pole, while others only run from the pole to the chromosomes. The final result is the chief thing, the actual method followed being subject to indefinite variety in detail, depending doubtless on the local conditions existing in the cell.

Again, the spindle, at any rate the part of it which lies between the two poles, may be entirely of nuclear origin, and it thus stands in sharp contrast with such a form as that existing in the Salamander. This kind of spindle occurs in *Ascaris meg.* var. *univalens*, in which the centrosomes are also intranuclear, also in the spermatogenic divisions in some birds, as was noticed by Moore a year ago. In plants it occurs in some fungi; in algæ Fairchild observed it in *Valonia*,[1] and it also occurs in *Fucus*. In the latter plant, indeed, it can readily be seen to arise within the nucleus, as the result of a differentiation of the achromatic substance, and the division of the chromosomes occurs while the original nuclear wall is still unbroken. This, to my mind, is strong evidence against the existence of any special spindle-forming substance, which is postulated by some writers. I believe the spindle to be nothing more than a differentiation in the preexisting protoplasm, and I see no need to further complicate a matter already sufficiently bristling with difficulties by in-

[1] Fairchild, "Ein Beitr. z. Kenntniss d. Kernth. b. *Valonia utricularis*," *Ber. d. Deutsch. bot. gesell.*, xii.

troducing any hypothetical substance which by assimilation and growth shall produce the body in question. The rapid differentiation, the exact correspondence in position with mathematically determinable lines of force, and the gradual transition of the radiations themselves into the general protoplasmic structure of the cell as well as the apparent indifference as to its cytoplasmic or nuclear or even mixed origin, are to my mind strong arguments in support of the contention that we need not yet fall back on a special substance—a sort of amorphous organ, as the *fons et origo* of the spindle fibres. It is not a little remarkable, however, to observe how very positive many people still seem to be as to the correctness of looking exclusively to either a nuclear, or to a cytoplasmic, or to some *special* source for the material of which the fibres are made up.

As we have met with so much variety in the character of the cell structures we have hitherto considered, we might be prepared to find differences extending even to the chromosomes themselves. And as a matter of fact we do. It is of course impossible to enter into details here, the subject is much too extensive; and I purpose only to mention those deviations which seem likely to cast some light on the obscurity which clothes the corporeal organisation which underlies, and perhaps is the cause of, what we term heredity.

It has become customary to regard the nucleus as the bearer of the hereditary qualities of the organism or of the cell, but the reasoning on which this view is based is not above reproach, for even in fertilisation a certain amount of cytoplasm is imported into the egg along with the male nucleus.

It is true that since the story of the fusion of male and female centrosomes during fertilisation seems to have become generally discredited, the interest attaching to the cytoplasm has again given way to comparative indifference in so far as questions of heredity are concerned; and, indeed, what experimental evidence we have appears all to tell in favour of the importance of the nucleus. Boveri's beautiful experiments, in which he fertilised non-nucleated fragments of the

egg of one species of echinoderms with the spermatozoids of another species, are a striking case in point. Boveri has recently repeated these observations, with the same result as before—namely, that he was able by adding spermatozoa to non-nucleated portions of ripe eggs to raise pure larvæ which resembled the *male* parent in every case, while in control series he found that when hybrids were formed with *nucleated* portions of ova, the resulting larvæ exhibited characters *intermediate* between the two parents.

As from the standpoint of heredity the nuclear divisions which are associated with the reproductive cells are of special interest, I will ask you to consider a few typical cases. And in the first place, one of the most striking and most uniformly recurring features connected with these cells lies in the numerical reduction to *one-half* of the previous number of the chromosomes. This reduction may be immediately associated with the differentiation of the sexual cells themselves, as is the case in animals, or it may occur at some other period in the life-history of the organism. Thus in all the Archegoniate and Phanerogamic plants which have been investigated with this object in view, the reduction is manifested in the first division of the spore mother cell; and if, as in the embryosac (macrospore) formation of *Lilium*, development takes a short cut, the reduced number becomes necessarily shifted to a later period than usual, but it manifests itself at the earliest time after these suppressed divisions would otherwise have occurred. Thus, in *Lilium*, the halved number of the chromosomes appears in the first division of the embryosac, whilst in *Allium*, in which the embryosac mother cell divides once (not twice) before forming the embryosac, the reduced number is apparent at this earlier stage. In the pollen grains, or microspores, the reduction likewise appears in the first division of the pollen mother cells. But it is a noteworthy fact that in *Fucus* the reduction takes place at a stage precisely corresponding with that in an animal, and this marks an important difference between *Fucus* and the Archegoniate plants in which the process has hitherto been most thoroughly investigated. It appears to indicate that

Fucus is probably a long way removed from the main series of plants, an opinion already shared by many people on other grounds. Indeed, it seems tolerably clear that there is nothing of the nature of that alternation of generations in *Fucus* which is so eminently characteristic of the main plant-series.

A phenomenon of such general occurrence as this of reduction is obviously one of profound importance, and it is scarcely to be wondered at that the divisions which are concerned in effecting it should have formed the subject of very many investigations. It was, and is still, hoped that some definite evidence might be forthcoming as to the way in which hereditary characters are transmitted and new qualities arise. For it has been tacitly assumed by nearly every one who has written on this subject that the teleological explanation of the whole process is in some way bound up with that of heredity. The only positive and bare inference however is that since the male pronucleus contributes the same number of chromosomes as the female a numerical reduction is necessary at some period in each sexual cycle, unless the number of chromosomes is to increase in geometrical progression in every succeeding sexual generation.

But with the general admission that a reduction is effected, all agreement between various investigators ceases. There is hardly less difference in opinion as to the manner in which the process is actually brought about than as to the meaning of the various stages. And once more, when we take a general survey of the facts, I think we see that the same result may be accomplished in a variety of ways; hence at present it is perhaps premature to lay too much stress on the details of this or that set of observations (however accurate they may be for the particular subject of investigation), in endeavouring to frame general theories. And so, though theorising is very inviting in such a subject as this, I think I shall best serve the purposes of this discussion by bringing before you some of what I conceive to be the most salient points characteristic of the reduction divisions in plants and animals, only criticising those views

which have been advanced when there appears to be definite evidence opposed to them.

One of the most remarkable facts which confront one in connection with chromosome reduction is that *two* divisions should be almost invariably associated with the process. The only exceptions I can recall are those in which, as in Lily embryosacs, the mitoses in which it would have normally been accomplished have been telescoped into a shorter course, owing to the suppression of the divisions of an embryosac mother cell.

And the second point lies in the rapidity with which the two divisions follow on each other; the daughter nuclei resulting from the first mitosis rarely go into the complete resting stage before they commence to divide once more. And this perhaps helps us to understand how the process may be so compressed as to admit of the telescoping of one or both divisions, such as is seen in the Lily.

The third point of general importance is the very long period of rest and growth through which the nuclei pass just before entering on these divisions. In pollen mother cells, and in the sporogonia of liverworts for example, the final number of spore mother cells is reached tolerably early, and then a very long interval elapses before they rush through the final mitoses. The same thing, though in a somewhat less degree, is to be seen in the reproductive glands of animals.

Fourthly, when the nuclei are preparing for the important divisions, after their long period of growth, they go through several remarkable phases, which are common both to plants and animals. I have seen them in all the cases I have investigated or had the opportunity of examining. The cells become extremely sensitive to injurious substances, and it is to this cause that the well-known difficulty of killing and fixing properly is due. Moveover, the linin framework in which the chromatin is increasing in quantity becomes lumped together in a highly characteristic manner; this is a transient phenomenon, and most people have been inclined to regard it as due to imperfect fixation. But I have myself seen it in the living cell in some animals, and

the same phenomenon has been also observed in living pollen mother cells.[1] But whether the appearance is connected with the process of reduction, or whether it is to be ascribed to some interaction between the linin and some of the other constituents of the nucleus, it would be unsafe to state definitely at present.

The fifth point lies in the early period at which the longitudinal fission of the linin filament becomes apparent, as compared with the ordinary dividing nuclei of the somatic cells.

The sixth point is the most important of all, namely, that as soon as the number of the developing chromosomes which are about to take part in this mitosis can be counted they are seen to be only one half the number visible at the corresponding stage of the previous nuclear generation, and this number is adhered to in the succeeding nuclear generations until, in the act of fertilisation, it again becomes doubled. Thus, however, we may finally explain it: the reduction in the number of the isolated chromosomes is seen to have been somehow or other brought about during the resting stage of the nucleus, or at any rate in the very early stages of mitosis. And this statement of the matter has been accepted with more or less reservation by most Botanists and Zoologists. Nevertheless we cannot blind ourselves to the fact that certain difficulties do exist in the way of its general application to cover all cases; some of these difficulties are matters of observation, others are questions of theory.

Finally, the seventh point lies in the very remarkable form assumed by the mature chromosome; it often appears as a closed ring which comes to be along the spindle, ultimately splitting across the middle. A careful study of the earlier stages leads however to the conclusion that we are nevertheless really dealing with a true longitudinal fission of the original chromosome rudiment, only the mutual relationship of the parts has been obscured by developmental

[1] E. Sargant, "The Formation of the Sexual Nuclei in *Lilium Martagon*," *Annals of Botany*, vol. x., 1896.

changes. It was owing to the peculiarities of these chromosomes in the Salamander that Flemming gave the name of *heterotype* to this mitosis to distinguish it from the nuclear divisions in the rest of the body. The term Heterotype, or Great Heterotype, has now come into common use to designate the *first* of the two divisions, because although the second *may* resemble the first one, as in Elasmobranch fishes according to Moore,[1] it far more commonly differs widely from it, approximating more or less closely to the form typical for the ordinary somatic nuclear divisions of the organism, as is the case in the Lily divisions. There are, of course, various forms under which the heterotype character of division may appear; but for the most part the net result is clearly a *longitudinal* division of the chromosomes followed by a second longitudinal division in the next mitosis.

I said "for the most part," because there are some apparently well-authenticated exceptions to the general scheme of division which I have just indicated, and they seem to show that in some cases at any rate the process of reduction may be accomplished in a very different way. In certain Crustaceans, Rückert[2] and some other investigators state that in the first of the two divisions with which we are concerned it is true that the number of the chromosomes *appears* to be reduced to one half, and that these undergo longitudinal fission in the usual way; but each of these single chromosomes is really double, and represents two of the original (pre-reduction) chromosomes which have become linked end to end together. The half number then may be said to occur here owing to the incomplete transverse separation of the linin thread, every other transverse separation having been omitted. But as the young (double) chromosome rudiments continue to grow their duplicate nature reasserts itself, and a sort of separa-

[1] Moore, "On the Structural Changes in the Reproductive Cells during the Spermatogenesis of Elasmobranchs," *Quart. Journ. Micr. Sci.*, vol. xxxviii., new ser.

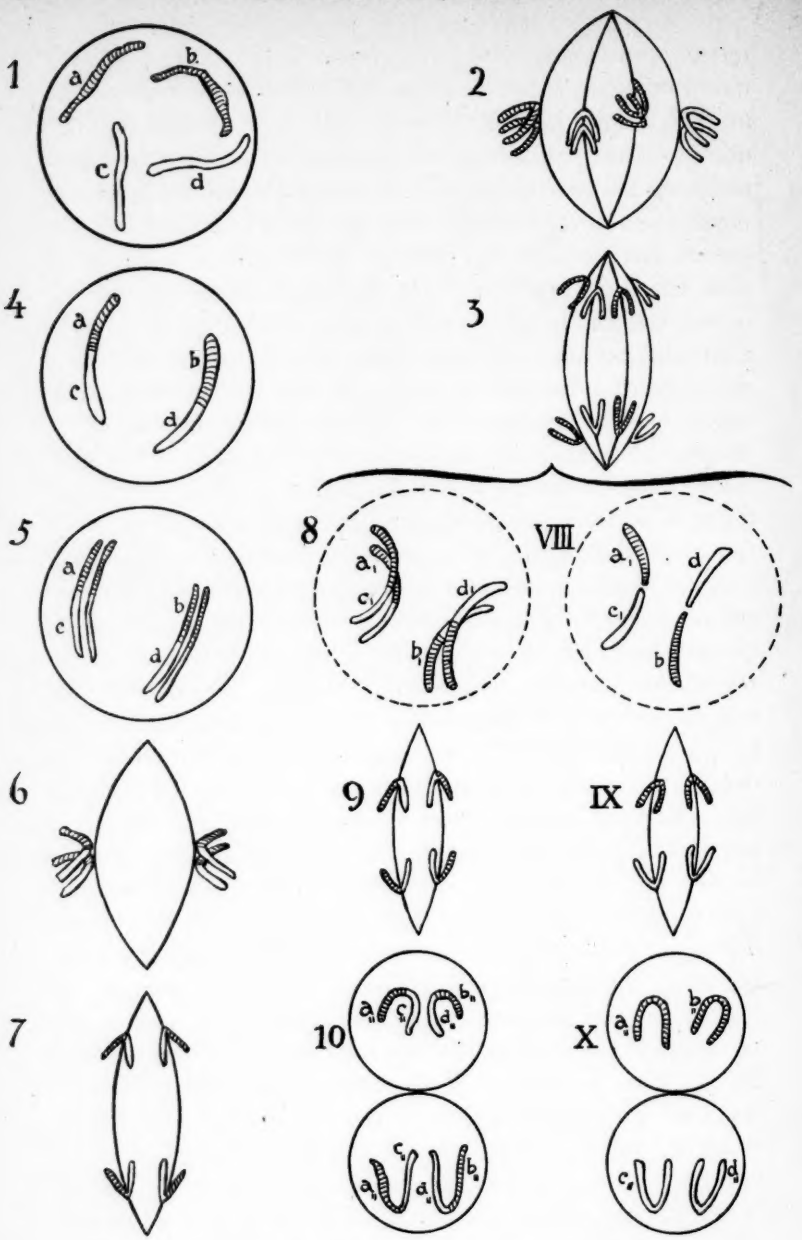
[2] Rückert, "Zur Eireifung bei d. Copepoden," *Anat. Heften, herausgegeben v. Merkel u. Bonnet*, 1894.

tion eventually takes place. Owing to the previous longitudinal fission and subsequent contraction, the position of each of the duplicate chromosomes is marked by the presence of four spheres. During the first mitosis these are distributed to the daughter nuclei in pairs. During the *second* division the two spheres in each couple separate in like manner, so that after the two divisions each of the four nuclei possess one of the original four spheres. Thus, in this case we are not merely dealing with a distribution of symmetrical quarters of *one* chromosome, but with a distribution of halves of *two entire chromosomes;* that is to say, if we for the moment adopt Weismann's standpoint, the hereditary qualities in this case are *divided* equally between the pair of nuclei which have received halves of *different* chromosomes, whereas if the reduction had been followed by two *longitudinal* divisions of one chromosome, *all* the hereditary qualities would have been distributed to each of the four nuclei.

It is clear that the former method of division, that described by Rückert for Copepods, exactly fulfils the requirements of Weismann's theory by removing one half of the material entities, in which he believes the hereditary qualities to reside, from each of the sexual cells. But it is obvious that, in order to admit the general validity of a reduction division such as seems to be warranted by a study of Crustacea, we ought to find *essentially* the same type of chromosome distribution recurring everywhere. Evidence is not, however, wanting to prove that by no means all organisms do conform to the crustacean type in these important respects, and even the most violent efforts have so far failed to fit them to this Procrustes bed.

In the divisions which result in the formation of the spermatozoids in *Ascaris megalocephala*, Brauer[1] discovered that, whereas in the antecedent mitosis, four chromosomes appeared in the nuclear spindle, in the first of the two divisions we are now considering, only two chromosome rudi-

[1] Brauer, "Die Spermatogenese von Ascaris megalocephala," *Arch. f. Mikr. Anat.*, Bd. xlii.



EXPLANATION OF THE FIGURES.

These figures are intended to illustrate the behaviour of the chromosomes during division. They are all purely diagrammatic.

FIGS. 1-3.—An ordinary vegetative mitosis.

" 4-7.—The first "reduction" division.

" 8-10.—The second "reduction" division as it occurs in Plants and in some animals.

" viii.-x.—The second "reduction" (true) division as it is said to occur in certain animals.

[Figs. 9 and ix. correspond to Figs. 3 and 7, and Figs. 10 and x. illustrate a later stage than is shown in the preceding series.]

ments were present, and that these were split lengthwise into four quarter chromosomes, by two longitudinal planes at right angles to each other. Thus the number of the chromosomes was *already* reduced, but the daughter chromosomes for this and the next division were provided for. According to Boveri, who is supported by other investigators, the same thing probably occurs also in connection with the formation of the ova in the same animal. Again, in the Salamander, as Flemming showed, the first division is a longitudinal one, although owing to the complexity introduced by the formation of closed rings, it at first sight might seem to be a transverse fission. That the second of the two divisions is longitudinal and not transverse is clear, inasmuch as it occurs while the daughter chromosomes of the first divisions are retreating to the poles. In this respect, Salamander offers a fine transitional stage between a normal case in which the splitting takes place at the commencement of the second division, and the instance quoted from Brauer, where the chromosomes undergo a *simultaneous* double fission which is thus effected for the two mitoses at one instead of at two stages. Finally, in the pollen mother cells of such plants as lilies, in which the details of the process have been most carefully worked out by a number of observers, it is clear that the chromosomes which appear in the first division spindle are reduced in number to one half, that these undergo longitudinal fission, and that in the next, and final division which follows rapidly on the first, a second longitudinal division also occurs. These observations have also been repeated on the division of the embryosac, and they have lately been again made the subject of an extremely careful investigation by Miss Sargant,[1] with the result that there is not the slightest evidence in favour of the reduction being in any way brought about by a sorting of the chromosomes as a whole; that is, the reduction as it occurs in these plants does *not* conform to the type as exhibited in the oogenesis and spermatogenesis of Copepods. But if it does not signify whether the chromosomes divide longitudinally or

[1] *Loc. cit.*

transversely, what becomes of the elaborate theories which have been founded upon the assumption of the universality of one or other of these two processes? The believed equivalence secured by a longitudinal fission was supposed to indicate a regular arrangement of the substance of the chromosome of such a nature that, although the composition might vary from one end to the other, the composition of any thin transverse plane could be assumed to be homogeneous. And the alternating discs of strongly and slightly stainable matter described by Balbani[1] in *Chironomus*, and seen by Strasburger in many plants, were thought to be the optical expression of this serial arrangement.

Since Weismann's theory has dominated a great part of cytological investigation, it has been assumed that this structure corresponds to a definite arrangement of the bearers of hereditary qualities. The chromosome is conceived of as a vastly complex structure, made up of small individual units which are grouped in a particular manner. But how does such a theory fit in with those facts concerning reduction which are now before us? It seems that if the chromosomes do really consist of a number of discrete bearers of hereditary qualities, of particles which must themselves be very stable, since the qualities they are assumed to bear are, as a matter of fact, remarkably constant;—it seems that the only way of making room for the fresh qualities introduced at the act of fertilisation is first to get rid of half the number of entire chromosomes from the sexual nuclei. This may be readily imagined in the case of Copepods, but how can we admit it in the other cases I have quoted? But in order to try to reconcile the doctrine of determinants and so forth, with the facts of reduction as exhibited in plants and, at any rate some, animals, it has been suggested that in the resting period before the first division the chromosomes really fuse end to end just as they are said to do in Cyclops, but that they never break asunder again. But this explanation lands us in all sorts of difficulties when we try to work it out. The determinants, biophores and so on, are, *ex hypothesi*, stable

[1] "Sur la structure du noyau des cellules salivaires chez les larves de Chironomus," *Zool. Anz.*, 1882.

bodies in themselves ; consequently, there must be present in each of the reduced chromosomes all the particles which were in the two originals. But it is clear that on this view no real reduction has taken place at all ; the new chromosome cannot be represented as C, but it is still AB and so on through the successive generations—this however is obviously impossible. But if it be urged that in the new chromosome, AB, there is a sort of struggle for existence amongst the hereditary units, half of which become ultimately extinguished, we are carried at once into the realms of pure fancy, and besides, why should not the units have had their fight out before ever the chromosomes were differentiated so that each chromosome in the reduced nucleus merely contains the survivors? But there is absolutely no evidence that any part of the nucleus does atrophy during these, rather than in other divisions ; indeed, the reproductive nuclei are characterised by their specially large size. Hertwig has tried to get over the difficulty by declining to recognise any such structural arrangement as Weismann and his adherents postulate ; for him the essence of the process lies in the rapid reduction of the nucleus to one fourth of its original bulk, by divisions at right angles to each other. Thus from Hertwig's point of view it does not signify whether these divisions are both longitudinal, or whether one is transverse,—the final result is the same.

And really this seems about to express our present knowledge on the subject. But if we accept such a view, it is difficult to see how we can help giving up a belief in the complicated architectural structure of the chromosome of which each constituent has been supposed to have its special position allotted to it, and its special part to play in the development of the organism. We come very near regarding the chromosome, complicated as it may well be in the structure of its molecules or molecular aggregates, as nevertheless intrinsically symmetrical. Heredity is then the outcome of its constitution *as a whole*, and is not the expression of activities residing in, and apportioned to, respectively discrete parts of it—assuming of course that the chromosomes *are* really the organising directors which

determine the qualities of the animal or plant in which they dwell. But any extended consideration of this topic would open up the question as to the real permanence of the chromosomes throughout the resting nucleus, in which they can no longer be discerned, and would involve an exhaustive discussion of the manner in which they emerge as distinct bodies at each recurring mitosis. And even to touch on the different questions and processes herein concerned would occupy an entire morning. And I think I have already taxed your patience severely enough without putting yet more burdens upon it.

I have tried in the foregoing account to point out how widely opinions differ on the most fundamental questions, the extremes being represented on the other hand by those who see in all organised structures special organs of the cell, which are permanent from generation to generation at least *in posse*, if not *in esse*, and which in some way regulate the changes which recur in cell life; the other side, on the contrary, regards the whole of cell phenomena, including the very "Organs" themselves, as the outcome and expression of forces which have their origin in profound chemical and physical changes as yet beyond our ken. But just as the operation of the forces themselves is transient if periodically recurrent, so also is the existence of some of the so-called organs themselves.

And perhaps in this, as in so many other matters, the middle way is the safest, but I have not sought to follow it because I did not consider it a suitable path for the purposes of a discussion.

"Serpit humi tutus nimium timidusque procellae."

J. Bretland Farmer.

SELECTION IN MAN.

(Continued.)

SINCE the publication of my last article on this subject, Dr. de Cazal has brought out an analysis of the French recruiting returns for seven recent years (1887 to 1893 inclusive).

Examined from the point of view I have been taking, *viz.*, the possibility of natural selection and modification of race-type by way of colour-change, De Cazal's figures do not afford much assistance. It is evident that there is now more laxity in admission, a lower standard of qualification, than was formerly the case; probably, moreover, there has really been an improvement in the physical health of the population generally. Anyhow, the releases on account of infirmities are not half so numerous as they were thirty years ago, while the releases on account of deficient stature have been done away with altogether.

The remissions for France in general averaging 100·4 per thousand, those for the 10 departments with the most blond population were 98·3, those for the 5 most distinctly presenting the combination of blond coloration, long head and tall stature which characterises the North European (Aryan?) type were 97·7, those for the 10 of darkest complexion were 106·8. Here there is no inferiority in the blond type. Let us now see how the exemptions for phthisis and for the euphemisms which often cover it are distributed.

No. of Departments.		Speciality.	Phthisis.	Scrofula.	Faiblesse de Const.	Total.
1	10	Highest stature	1·84	1·75	5·26	8·85
2	10	Lowest ,,	2·81	3·36	7·06	13·23
3	10[1]	Blond	1·97	2·28	5·51	9·76
4	10[1]	Dark	1·59	2·31	6·02	9·92
5	5[1]	Tall, Blond, Dolich	2·52	1·94	6·64	11·10
6	6	Urban	2·88	2·85	8·50	14·23
7	10	Mountainous	1·83	2·52	6·83	11·18

[1] I have excluded the densely peopled departments of the Nord, Seine Inferieure and Bouches du Rhone.

There is little here to guide us, though the somewhat unfavourable aspect of column 5 is a little suspicious. It may be noted, by the way, that a general high level of stature does not seem to entail a high proportion of phthisical disease, though I have little doubt that in France, as with us, tall young persons suffer more therefrom than short ones in the same community. Knox, who must already have grasped the idea of modification of type by selection, said long ago that the South African Boers were taller than the natives of Holland because among the former the taller youths were not so liable to be cut down by phthisis.

To return—Roberts and Rawson, in their anthropometric report to the British Association, compared the complexional characters of various periods of life, from infancy to extreme old age. The gradual deepening of colour, of hair colour especially, which concurs in most individuals with the advance of years, the earlier age at which greyness is perceptible in the dark haired, and some considerations of minor importance, take away much of the value of Roberts and Rawson's table, carefully digested and widely based though it be. *Primâ facie*, however, it may be claimed as evidence that the changes in colour which do occur are due to development rather than to selection by death.

The possible effect of conjugal selection has been tested by myself, but very insufficiently. My observations tended to show that, among the working classes of Bristol, women with red and women with black hair did not marry in quite so large a proportion as those with hair of other colours. If this were to continue through several generations, we should have a diminution of the two most abundant kinds of hair-pigment (Sorby), and a general prevalence of dull shades of brown. De Lapouge[2] quotes De Candolle to the following effect: 158 brown-eyed men espoused 52 per cent. of women of like colour with themselves, and 48

[1] *Races of Britain*, p. 226.

[2] *Les Selections Sociales*, par G. Vacher de Lapouge, p. 202, Paris, 1889.

per cent. of women with blue or grey eyes. Now there are 44 per cent. of brown eyes among the women of French-Switzerland, so that it seems as though the brown eyes were more attractive. Similarly, 167 men with blue or grey eyes married 59 per cent. of brown-eyed women, which is much above the proportion in the women of the country.

De Candolle[1] found that while 72 couples having both dark eyes gave birth to 3·07 children on an average, 131 couples having both light eyes had only 2·72. He notes that children who were under 10 years of age at the period of observation were not counted; but this fact detracts very little from the value of the observation, which would lead one to expect the rapid diminution of light eyes, and probably also of the blond type. The number of cases, though considerable, is perhaps hardly large enough to support a conclusion of such magnitude. And Mr. Galton, on the other hand,[2] dealing with a much larger number of cases, comes to the conclusion that he may "disregard the current popular belief in the existence of a gradual darkening of the British population," and regards eye-colour as "statistically persistent". Mr. Galton's method of working, however, is backwards, from children to parents and grandparents, and therefore takes, I apprehend, no account of the number of barren couples, which, if we could ascertain it, might possibly modify this conclusion. His figures are as follows (summarised):—

	Males and Females.			
	Blue.	Grey.	Neutral.	Dark.
First Generation	35·9	27·3	10	26·6
Second ,,	29·4	31·3	12·1	27
Third ,,	28·5	32·3	14·7	24
Children	29·6	28	19·4	23·3

Now Mr. Galton, by an examination of the pedigrees of neutral-eyed persons, finds that they may fairly be taken as the offspring, roughly speaking, of parents of whom two-thirds had light and one-third dark eyes. And if we

[1] *Revue d'Anthropologie*, p. 265, 1887.
[2] *Natural Inheritance*, cap. viii., and table xv.

divide the neutrals on this principle, we shall find that we have in the first generation 29·9, and in the last 29·8 of dark hair.

It is often asserted that the milk of brunets is richer in solids than that of blonds; but I do not think the statement, though not improbable, rests on any sound foundation.

So far our inquiry leads one to suspect a certain degree of physical inferiority in the blond constitution, which may possibly be operating towards the gradual diminution of this type. Let us now try a comparatively uncultivated branch of the subject.

I have examined a large quantity of material derived from the serial volumes of the *Police Gazette*, or *Hue and Cry*. These contain the lists of deserters from the Army, Militia, Marines and Royal Navy, with their names, stature, complexions, occupations and alleged birthplaces. I must allow, *in limine*, that these deserters are themselves the product of successive selections, and are therefore not altogether trustworthy representatives of the general or lower-class population. They are not conscripts, but volunteers; moreover, these volunteers have been again sifted by the medical rejection of the physically unfit, and these deserters are the product of a third selection implying possibly moral if not physical qualities of a kind differing from those of the soldiers who do not desert. Still, after all this has been said, there is a good deal of internal evidence to lead one to think of them as not a mere heterogeneous collection, but something approaching to a sample of the classes they belong to.

I have taken great pains to eliminate the personal equation of the medical observers, by using the returns for a number of years, scattered over the period from 1845 to 1885, and have rejected any returns which seemed to bear evidence of careless and slovenly observation.

I have also abstracted and analysed the lists of convicted or suspected criminals, of runaway convicts during the period of transportation to Australia and Tasmania, and of men discharged from the Army with disgrace.

My results, if not striking, are curious. Thus, classifying the deserters by their occupations, we have :—

		No.	Index of Nigrescence.	Prop. of Light Eyes.
All Occupations	England	8380	+ 5·57	61·1
	Wales	700	+16·85	56·6
	England and Wales	9180	+ 6·37	60·8
	Scotch	1733	+ 8·08	72·2
	Irish	2275	+18·85	70·2
	Irish by name, born in Great Britain	733	+22·24	69·8
	England (later series)	3500	+ 5·90	61·7
Grooms	English	300	− 5·30	70·3
Butchers		72	− 6·90	66·6
Smiths, Carpenters, etc.	English and Welsh	250	+ 6·6	63·6
Clerks	English	125	+ 2·4	54·4
Do.	English, Welsh and Scotch	400	+10·2	59·7
Tailors and Shoemakers	English and Welsh birth and names	425	+13·4	57·8
Country-born Labourers	English (pure), largely from S.E.	1010	+ 1·8	60·1
Country-born Artisans	Do.	340	+ 7	58·4
Town-born Labourers	English (pure)	550	+ 6·3	64·3
Town-born Artisans	Do.	300	+10	59·3
Irish-born Labourers	Irish	400	− 3	74·5
Do. of other Occupations	Irish	180	+15	70·5
English-born Labourers	Irish by name	60	+16·6	75
English-born other Occupations	Do.	65	+23·1	75

The index of nigrescence is gotten by subtracting the instances of red and of fair hair from those of dark brown and of black, doubling the black on account of its greater potency. Thus (2 Black + Dark Brown) − (Red + Fair) = Index. The brown are omitted as neutral.

Examination of this table raises a presumption that there is some kind of moral selection or natural bias, which induces a certain number of the dark-complexioned men to betake themselves to sedentary and indoor employments, while

certain blond prefer the outdoor employments connected with the land or with the care of animals. The case of the grooms is pretty distinct and founded on a tolerably broad basis, and both it and that of the butchers agree with my personal observations. The passion for "horse-flesh" and skill in the management of horses does seem to be in some degree hereditary and even racial. The Scandinavians had it, though the natural conformation of their native country was unfavourable to its indulgence; and Australian blacks, who have been but just introduced to the animal, speedily become expert and daring riders.

The apparent difference between the Irish labourers and artisans deserves some comment. The numbers are of course rather small—only sufficient to raise a suspicion that they indicate a fact. The difference assuredly does not represent that of Catholic and Protestant, of exotic and indigenous Irishman. For, as I have shown elsewhere, that difference is of the opposite kind, the exotic, who is often Protestant, being slightly oftener blond than the aborigine, who is almost always Catholic. The men here described as "Irish-born labourers" are largely of the peasant or small-farming class. The "English-born Irish" are probably in race-type *ipsis Hibernis Hiberniores*, for having all true Irish surnames they are of indigenous Irish and not of exotic descent, so far at least as surnames are a guide, which in this particular case they are to a considerable extent.[1]

I have chosen tailors and shoemakers as specimens of the sedentary artisan class, and as being tolerably evenly distributed throughout the country, though the shoemakers of late years, since the partial concentration of the industry in a few large towns, are less so than the tailors. A comparison of tailors from Cornwall with grooms from Yorkshire or Norfolk would be of little avail. It will be observed

[1] It is not simply that a Sullivan or an O'Donnell or a Connellan ***must*** be Irish by remote paternal descent; that is little; but that as exotic surnames are comparatively few in the western half of Ireland, and as religious differences count for something in matrimony, the maternal is likely to have very largely resembled the paternal ancestry.

that the tailors and shoemakers include a large proportion of dark men. I have tested also to some extent the painters, plumbers, etc., the weavers, the colliers, the smiths and carpenters. The first seem to yield figures resembling those of the tailors; the colliers include many blond men; but they and the weavers are unsatisfactory by reason of their local distribution. The number of the middle and upper class recruits is small; they are included here among the clerks. The higher classes in England and Ireland, so far as my observations go, are rather on the blond side of the general average of the populations, but the present investigation yields nothing in support or explanation of this.

"Legal selection," says De Lapouge, "ought to operate so as to prevent the criminal from mischief, and to suppress his posterity." Of course at present we do this very imperfectly; and our failure to do it has and will long have disastrous effects; still our action has probably some force in that direction. That certain physical types tend towards criminality is generally acknowledged, though some criminal anthropologists may be too hasty in defining them. In this country more criminals than honest men are of dark complexion. Thus Roberts and Rawson[1] supply material for the following table, which includes the whole British Isles—

	Index of Nigrescence.	Prop. of Light Eyes.
General population[2]	+ 1·0	66·2
Criminals in prison	+ 8·3	56·4
Lunatics in asylums	+ 5·1	66·9

Here the criminals have about 54 per cent. of dark hair against 50½, and 10 per cent. more of them have dark eyes. Red hair appears in smaller proportion than is usual. These characteristics run through the statistics of the four countries without much variation.

The following data are abstracted and summarised from the *Police Gazette* for several years :—

[1] *Final Report of the Anthropometric Committee*, p. 21.

[2] These figures are not strictly comparable with those I have been analysing, the mode of collection having been different.

CRIMES AGAINST PROPERTY, OR WITHOUT VIOLENCE.

1.	Nationality.	Index.	Light Eyes.	[1]Neutral Eyes.
250 Male Criminals	English or Welsh birth and name	+28·2	62·8	
500 ,, Supervisees [2]		+21·2	59·4	4·0
100 ,, Supervisees	English, pure	+14	49	
200 ,, ,,	In England, but of all births	+16·5	62·5	
300 ,, ,,		+14·6	58·3	6·3
100 ,, ,,	Irish in England	+19	68·2	2
100 Juvenile Males	English	− 1·5	55·3	
100 Adult Females	English	+13	49	
200 ,, ,,	All nationalities	+16·5	62·5	2·5
100 ,, ,,	Irish in England	+25	68	2

The evidence of this table, so far as it goes, runs in the same direction as that of Roberts and Rawson's report. It will be observed that the inclusion of Scotchmen tends to increase the proportion of light eyes, while that of Irishmen generally increases also the index of nigrescence. The figures relating to juvenile criminals are significant, these being often so by hereditary right. The hair index is light, owing to the immaturity of the subjects; but the large proportion of dark eyes is remarkable. Possibly, however, this may be due to their being largely natives of the slums of great cities. I cannot test this suggestion, having no data relating to homologous children not criminal.

The next table has been constructed from records of the colour of the hair and eyes of transported convicts, who had escaped from their allotted place of service during several years previous to and including 1852.

CRIMINAL FUGITIVES.

		Index.	Light Eyes.	Neutral.	Dark.
200 Males	All Nationalities	+27·5	43·5		56·5
500 ,,	English, Welsh, Scotch	+31·2	52·6		47·4
400 ,,	English	+28·3	44·2	14·2	47·6
30 ,,	(Included above) London	+43·3	30		70
50 ,,	Irish	+42	66	8	26
50 Females	All Nationalities	+46	36		64
87 ,,	English, Welsh, Scotch	+43·9	58·5		41·5
21 ,,	Irish	+62	66·6		33·3

[1] Most of the observers seem to classify these among the dark eyes.

[2] Persons kept under supervision by the Police after discharge from prison.

These unfortunates, who may be taken to represent generally the quintessence of criminality, show a great tendency to darkness in both eye and hair. This is perhaps least marked in the Irish men, who also average more than an inch in stature beyond the rest. No doubt a larger proportion of them did not belong to the professional or hereditary class.

The subject of convict genealogy is a sore one with our Australian cousins, who certainly did all they could to cut off the current at its source by refusing to receive fresh supplies. The (probably) prevalently blond types of the free immigrants seem to have asserted themselves victoriously. The typical bushman is tall, blond rather than dark, and freckled. Of 35 Australasian deserters the index of nigrescence was − 8·5; and the proportion of light eyes 57·1 per cent.

CRIMES OF VIOLENCE.

			Index.	Light Eyes.	Neutral.	Dark.
88	Males	English and Welsh, a few Scots	+36·3	46·6		53·3
237	„	„ including the above	+40·1	not stated		
70	„	Irishmen	+42	60	2	37
67	„	Rape, and similar offences, all Nationalities	+37	62		38

In the last category both red and black hair are strongly represented; in the others black, but not red. Nothing like this is observable in any of the lists of criminals against property or without violence; and I do not think this discrepancy can be accidental. The average stature in the violent criminals seems to be good, but in many cases it is estimated, not measured, and estimates of this kind are more often in excess than otherwise.

Yet another class of evil-doers, not precisely criminal, however, may be found in the men discharged from the army "with ignominy," or "as worthless and incorrigible". A series of these, numbering 200, yielded me an index of nigrescence of 7 per cent., and 63 of them had light eyes. They evidently differ from the ordinary criminal classes:

many of them are blond, and their average stature is fully up to or over the standard of ordinary deserters, and of course very much over that of ordinary criminals. Apparently the men who give the prevailing character to this class are drunkards, unstable, untrustworthy, insubordinate, have in fact the vices of the sanguine temperament, while the typical violent criminal, with his dark complexion, is choleric or melancholic, and the typical perpetrators of sexual offences are choleric or sanguine.

Criminal anthropologists as a rule assign great importance to one or other of two hypotheses, that of atavism and that of degeneration. Lombroso might derive some support from my facts for the former belief. Mr. MacRitchie, who thinks he has proved the historical existence and numerical importance, in earlier days, of whole tribes of outlaws of gipsy type, might deduce some of my criminals from them. Something of the sort may apply to the criminals of violence, to enough of them, that is, to give the apparent colour to the whole mass. But the ordinary criminal, so far as he is not the product of contemporary surroundings, looks more like a product of degeneration. The mean stature is conspicuously low both in my ordinary malefactors, and in the Australian fugitives. As to the kephalic development I have unfortunately no data, and it is probable that in this country at least it is less characteristic than the facial physiognomy, of which Mr. Galton has given us some very striking composite photographs. Meanwhile, we must wait to see what Dr. Garson's investigations may lead to. What is most required, and what most criminal anthropologists have somewhat neglected, is to ascertain the ordinary physical standard of the classes whence criminals arise, and thus to obtain a basis of comparison.

The effect of what De Lapouge styles "legal selection," that is, the temporary or permanent incapacitation of the criminal to propagate his kind, is at present very small. During many years, and indeed many generations, attempts were made to eliminate the criminal type from the British Isles, at the risk of its propagating itself with augmented

rapidity in America or Australia. I have already said that in the latter country the attempt does not seem to have been so perniciously successful as might have been anticipated or feared. And in our own islands social conditions seem to step in where legal ones fail, and the criminal by degeneration is not as a rule highly reproductive. Here we seem to have discovered an agency which may tell a little in favour of the blond, who is menaced with gradual extinction by other processes.

The kephalic index, however, is a much more promising test in matters of selection than the colour of the hair and eyes. In England, indeed, less can be expected from it than in some other countries, owing to the fact that most of *our* race-types are more or less dolichokephalic, and therefore not to be readily distinguished by the simple application of the callipers. But De Lapouge in France, and Ammon in Belgium, have developed and worked this method with ingenuity and success, some of their results being unexpected and indeed wonderful. Of these I hope to give an account in my next communication.

JOHN BEDDOE.

THE GLOSSOPTERIS FLORA; AN EXTINCT FLORA OF A SOUTHERN HEMISPHERE CONTINENT.

THE last phase of the Carboniferous period in the Northern hemisphere was marked by widespread forests stretching over a considerable tract of country, intersected by shallow lagoons or the remnants of a former sea, on the floor of which were built up thick masses of limestone in the earlier part of the Carboniferous Epoch. A succession of sandstones, shales and coal seams, marks the position of this ancient forest, and from the fossil plants of the Coal Measures we learn that the vegetation was characterised by a general uniformity in the plant types. Gigantic Vascular Cryptogams, such as *Calamites*, *Lepidodendra*, *Sigillarieæ*, numerous ferns, and extinct forms agreeing in the structures of their wood with present-day coniferous and cycadean plants constituted the greater part of this Palæozoic flora. In the succeeding Permian period the vegetation retained in many respects the character of the Coal Period forests; some genera had become extinct, and others became more abundant or appeared for the first time. From Arctic latitudes, through Europe, North America and China the fossil remnants of this Permo-Carboniferous vegetation have been found in abundance, and from the Zambesi district of Africa typical Coal Measure genera are recorded.

Passing further south, we find in South America, Africa, India and Australia, sedimentary deposits which it is believed represent fragments of a Southern hemisphere Continent. In these beds certain forms of plants are met with in considerable abundance, and the botanical evidence distinctly points to the existence of a widely distributed flora differing in its most characteristic types from the contemporaneous vegetation of the North. The Southern plant beds are in part of Permo-Carboniferous and Triassic age.

With one or two exceptions, the Northern Palæozoic forms are not met with in this Southern vegetation. In Australia, more particularly, a few well-known Carboniferous plants have been found, but these belong to a period represented in Europe by rocks anterior to the Coal Measure epoch.

In South America and lately in South Africa one or two Coal Measure genera have been discovered, but, generally speaking, the flora of this Southern Continent is characterised by such genera as *Glossopteris*, *Gangamopteris*, *Nœggerathiopsis* and other plants, some of which are absent from the Permo-Carboniferous vegetation in the Northern hemisphere, and others play but a very insignificant part in the composition of the flora. The object of this article is to give a brief summary of this Southern vegetation.

In 1828 the French palæobotanist Brongniart[1] figured and described some tongue-shaped and reticulately veined leaf impressions under the generic name of *Glossopteris*.[2] These fossil leaves are abundant in Australia, and had previously been regarded by travellers as specimens of the well-known *Eucalyptus*. Some of the species included by Brongniart in this genus have since been transferred to other genera, *e.g.*, *Glossopteris Phillipsii* Brong. from the Lower Oolite of the Yorkshire coast is now known as *Sagenopteris Phillipsii* (Brong.). The type species, *G. Browniana* Brong., still remains one of the best-known examples of the genus. The specimens on which this species was founded were from Australia and India, and Brongniart speaks of the leaves from the former country as *G. Browniana* var. α *Australasica*, and those from the latter as *G. Browniana* var β *Indica*. He describes the Australian forms as smaller in size, subspathulate and obtuse, and the Indian leaves as larger and possessing a lanceolate

[1] Brongniart (1), p. 222, pl. lxii.

[2] He defined the genus as follows: "Folia simplicia, integerrima, sublanceolata, basi sensui angustata, nervo medio valido apice evanescente percursa; nervulis obliquis arcuatis æqualibus, pluries dichotomis vel basi quandoque anastomosantibus et reticulatis".

and acute apex. *Acrostichum*, *Aspidium*, and other recent ferns are compared with the fossil genus, and in one example of *Glossopteris* certain ill-defined spots are spoken of as traces of rounded groups of sori. Since Brongniart's diagnosis was published thousands of *Glossopteris* leaves have been discovered in India, Australia and Africa, but we are still in doubt as to the precise affinities of the genus. Several writers have described markings on leaves which they refer to as sori, but it is extremely doubtful whether these supposed fructifications are of any botanical value. In 1861 Bunbury[1] described some leaves of *Glossopteris Browniana* from India on which were "small round spots very regularly arranged in one or two rows parallel to the margin". The specimens figured by Bunbury are in the Museum of the Geological Society of London; an examination of the fossils shows that the supposed sori have the form of irregularly circular holes in the leaf lamina, bordered by a narrow brown rim, and distributed with but little regularity. The figures 1 and 4 of Bunbury's paper (pl. viii.) convey a very fair idea of the appearance which he describes.[2] It may be that the patches are merely holes in the leaf, and not the impressions of sori, but it is impossible to speak very decidedly as to their real nature. In the absence of any trace of sporangial impressions, we cannot accept the specimen as affording any trustworthy evidence of the nature of the fructification of *Glossopteris*.

In 1872 Carruthers described an Australian form of the same species, in which he noticed "some indications of fruit in the form of linear sori running along the veins and occupying a position somewhat nearer to the margin of the frond than to the midrib".[3] Unfortunately no figure is given of this supposed fertile leaf. Some years later Feistmantel described another type of fructification in which "the secondary veins quite close to the leaf margin seem to pass into an intra-marginal longitudinal vein, leaving thus a very narrow empty space along the margin, which perhaps

[1] Bunbury. [2] No. 10,356 in the Geological Society's Collection.
[3] Carruthers (2), p. 6.

shows that in fertile fronds the fructification was a marginal one, like the fructification of *Pteris*".[1] On the strength of these discoveries, Feistmantel instituted three groups of *Glossopteris* founded on the nature of the sori. Carruthers' specimen of *G. Browniana* from Australia is the type of Group 1, and is compared with the recent genus *Anthrophyum;* of Group 2, *G. indica* Schimp. is the type, and is compared with *Polypodium*, while Group 3 is represented by Feistmantel's examples of *G. augustifolia* with the Pteris-like fructification. It has recently been pointed out[2] that this classification of Indian and Australian forms has been further complicated by the discovery of the Indian type of sori in Australian leaves.[3] The fragment responsible for this supposed complication is imperfectly preserved and shows on its surface three "sub-oval convex impressions," which cannot be accepted as genuine traces of sporangia. In referring to this discovery, Etheridge remarks that the supposed sporangial groups do not convey to him the idea of sori, but adds: "We must however bow to the evidence, such as it is, and it would thus appear that we have in Australia Glossopteris-like leaves possessing two out of three forms of fructification".[4] If evidence is unsatisfactory it is hardly a matter of duty to accept it, or to deduce from it far-reaching conclusions! On the whole, it would seem the safer course to admit that as yet no trustworthy example of a fertile *Glossopteris* frond has been recorded from either India or Australia, and we have certainly no data on which any classification can be legitimately founded.

Until we possess leaves showing distinct sori and sporangia it is of little use to institute a comparison of the sterile fronds with those of recent ferns. The leaves of species of the polypodiaceous genus *Anthrophyum* agree very closely in form and venation with *Glossopteris;* in some species from Fiji there is little or no midrib; in others from South America the midrib may extend to the leaf tip. In the fertile fronds the sporangia occur in continuous lines,

[1] Feistmantel (2), p. 97.
[2] Etheridge (4).
[3] Mitchell.
[4] Etheridge (3), p. 237.

following the course of the veins on the under surface of the lamina, such a frond imperfectly preserved as an impression would probably convey the idea of fairly broad veins rather than of rows of sporangia. The genera *Acrostichum*, *Hemionitis* and others afford close parallels with *Glossopteris* as regards shape and venation.

Another well-known fossil often associated with *Glossopteris* is the genus *Vertebraria*, founded by Royle in 1829.[1] He proposed this name for some Indian fossils which are figured in his work on the *Natural History of the Himalayas*, and described the specimens under two specific names, *V. indica* and *V. radiata*. No real specific difference can be detected in the examples figured. Bunbury, Feistmantel and other writers have given more complete descriptions of this genus; a typical specimen usually presents the appearance of a long cylindrical structure varying in breadth from a quarter of an inch to an inch, occasionally branched, and the axis marked by a median line from which offshoots pass off at intervals to the margin of the fossil.

Bunbury was the first to suggest that the *Vertebrariæ* might be the roots of some large plant.[2] The specimen on which Bunbury's opinion was based is in the Museum of the Geological Society; it shows the numerous roots remarkably well and the figure hardly does justice to this unusually interesting example of the genus.[3] Until quite recently we have been absolutely in the dark as to the nature and relationship of this fossil. Zeiller[4] has now brought forward good evidence that *Vertebraria* is the rhizome of *Glossopteris*. From an examination of specimens recently found in the Beaufort beds of South Africa he has been able to trace a connection between *Glossopteris* and *Vertebraria;* he finds that the median veins of the former are continued as transverse grooves in the axis of the latter. The median line occupying the axis of a *Vertebraria* would thus be the central stele and the lateral offshoots leaf-

[1] Royle, p. xxix.*, pl. ii.

[2] Bunbury. The specimen figured by Bunbury (pl. xi., fig. 3), now in the Geological Society's Museum, shows the roots remarkably well.

[3] Bunbury, pl. xi., fig. 3. [4] Zeiller (5).

trace bundles. There are also reasons for believing that the stem of *Glossopteris* bore two kinds of leaves, the ordinary tongue-shaped fronds and much smaller scale leaves. Zeiller compares *Struthiopteris Germanica* and *Oleandra* with *Glossopteris*, both as regards the arrangement of leaves on the stem and the distribution of vascular strands in the rhizome.

In spite of the fact that in some districts beds of rock extending over a wide area are literally made up of *Glossopteris* leaves, these are almost without exception detached fronds and not attached to any stem structure. In Africa beds of shale are frequently found to be almost entirely made up of *Glossopteris* fronds, and in Australia[1] beds of clay have been described as owing their foliation to the close packing of the leaves of this fern. In 1849 Dana[2] figured an example in which several fronds appeared to be given off from a stem fragment, and another specimen has now been figured by Etheridge[3] from New South Wales. In the latter case several lanceolate fronds, which agree in outline with *Glossopteris linearis* McCoy, and *G. Clarkei* Feist., are attached to the apex of a comparatively broad stem. The surface of the stem is covered with crowded leaf-scars, and an internal cast shows numerous narrow protuberances which probably represent the leaf-trace bundles. The general appearance of the stem does not agree at all closely with that of *Vertebraria*, which Zeiller has shown to be the rhizome of some forms of *Glossopteris*. It is probable, therefore, that there was more than one form of rhizome bearing true *Glossopteris* leaves.

The wide distribution and extended geological range of *Glossopteris* in the Southern hemisphere is a fact well known to geologists, and of considerable interest from the point of view of the distribution of fossil plants, and the development of ancient floras. The term Glossopteris flora originally proposed by Neumayr, has come to be employed for an assemblage of plants ranging from the Upper Palæozoic to Lower Mesozoic strata, and char-

[1] Dana, p. 716. [2] Dana, pl. xii., fig. 13 C. [3] Etheridge (3).

acteristic of the Southern hemisphere. Feistmantel[1] has objected to the use of the term Glossopteris flora on the ground that the genus *Glossopteris* extends to the Rhætic and Jurassic beds, and is not confined to the Permo-Carboniferous rocks which are usually the strata to which Neumayr's term is applied. With the exception of a species of *Glossopteris, G. Browniana*, var. *Australasica*, described by Zeiller from the Rhætic beds of Tongking,[2] no typical example of the genus appears to have been recorded from any locality north of Northern India. A very imperfectly preserved impression figured by Trautschold from Russian Lower Cretaceous beds under the name of *Glossopteris solitaria*[3] affords no proof whatever of the occurrence of that genus. The Tertiary plant referred to this genus by Visiani and Massalongo in 1858[4] cannot be accepted as an undoubted example of this Southern hemisphere fern.

Having given some account of the typical member of the *Glossopteris* flora, we may pass on to describe very briefly the most important districts in which this assemblage of plants occurs.

India.—Occupying a vast area in Bengal and Central India, there is a series of beds of fluviatile origin, containing few animal remains, but fossil plants in abundance. This enormous mass of river sediments constitutes what Medlicott has termed the Gondwana series, the name being chosen from the occurrence of the rocks in a district south of the Narbadá Valley, which it is supposed was formerly inhabited by the aboriginal Gond tribes.[5] In speaking of this series in his presidential address at the Montreal meeting of the British Association, Blanford[6] draws attention to the unusual interest of the Gondwana beds "on account of the extraordinary conflict of palæontological evidence" afforded by the fossil plants and animals. The Gondwana beds are divided into two groups, the Lower and Upper, distinguished from one another by well-marked

[1] Feistmantel (5), p. 13. [2] Zeiller (1). [3] Trautschold, pl. xix., fig. 1.
[4] Visiani and Massalongo, p. 206, pl. i., fig. 1. [5] Oldham.
[6] Blanford (1), p. 696.

stratigraphical and palæontological breaks. It is with the lower divisions that we are at present concerned; this includes three series, at the base the Talchir and Karharbári beds, followed by the Damuda and Panchet series.

LOWER GONDWANA.
- Panchet.
- Damuda.
  - Ranigunj and Kámthi.
  - Barákár.
- Karharbári.
- Talchir.

The Talchir and Karharbári beds consist of shales, sandstones, conglomerates and coal seams. Towards the base of the series boulder beds occur over a wide area, made up of fine silt full of rounded and subangular striated rocks, varying in size from ¼ inch to 15 feet in diameter. In some places the boulder beds rest on a platform of older rocks exhibiting the polished and striated appearance of an iceworn surface.

The following species of plants may be mentioned from this horizon :—[1]

Schizoneura cf. Meriani, Schimp.
Vertebraria indica, Royle.
Gangamopteris cyclopteroides, Feist., and other species of the genus.
Glossopteris Communis, Feist.
G. indica, Schimp., and other species.
Glossozamites Stoliczkanus, Feist.
Nœggerathiopsis Hislopi (Bunb.).
Euryphyllum Whittianum, Feist.
Voltzia heterophylla, Brong.

Damuda series.—This important group of rocks consists of sandstones, shales and seams of coal, from which a fairly rich flora has been obtained. The following may be given as characteristic species :—[2]

Schizoneura gondwanensis, Feist.
Phyllotheca indica, Bunb.
Trizygia (*Sphenophyllum*) *speciosa*, Royle.
Vertebraria indica, Royle.
Sphenopteris polymorpha, Feist.
Alethopteris Whitbyensis, Göpp.
Macrotæniopteris danæoides, Royle.
Glossopteris communis, Feist.
G. Browniana, Brong., and other species.
Gangamopteris cyclopteriodes, Feist., and other species.
Belemnopteris Woodmasoni, Feist.
Sagenopteris polyphylla, Feist.
Pterophyllum burdwanense, McCl.
Nœggerathiopsis Hislopi (Bunb.).
Voltzia heterophylla, Brong. Etc., etc.

[1] For figures of these plants see Oldham and Feistmantel (1).
[2] See Feistmantel (2).

Of the above fossils, *Glossopteris* and *Vertebraria* are the most abundant, Cycads and Conifers being exceedingly rare.

From the overlying *Panchet* group, several fossil animals, but few plants, have been recorded:—

Schizoneura gondwanensis, Feist. *Glossopteris communis*, Feist.
Vertebraria indica, Royle. *G. indica* and other species.

In the extra-peninsular area boulder deposits and fossiliferous marine strata occur in the Salt Range, N.W. Afghanistan and other districts homotaxial with the Lower Gondwana Rocks of the Indian Peninsula. The Salt Range beds[1] are of special interest as having afforded trustworthy evidence as to the geological age of the Lower Gondwana strata; from the marine beds typical Carboniferous fossil shells have been discovered of the same species as Australian and European forms. Of these, *Conularia lævigata*, *Conularia tenuistriata*, *Plemeotomaria nuda*, *Conetes cracowensis* and other species occur in the Salt Range and in marine strata of New South Wales. The question of the geological age of the Salt Range beds has given rise to considerable controversy, but now it is generally admitted that the boulder beds of this area and those of the Talchir series are homotaxial, and of Permo-Carboniferous age.[2]

AUSTRALIA.[3]—In New South Wales, Victoria, Queensland, Tasmania and other districts the Glossopteris flora is abundantly represented, and, as in India, glacial beds are associated with the lowest strata containing members of this flora. Confining our attention to rocks below the Mesozoic System, the beds have been classified as follows in New South Wales:—

Wianamatta Shales.
Hawkesbury Sandstones.
Newcastle beds, or Upper Coal Measures.
Upper Marine beds with Carboniferous fauna
Stony Creek beds, or Lower Coal Measures } Muree beds.
Lower Marine beds with Carboniferous fauna

[1] Oldham, p. 121. See also Waagen, Blanford (2) and (3), Seward, etc.

[2] *Phyllotheca* and *Vertebraria* are recorded from the Sarawak Coal in BORNEO. (Posewitz, *Borneo*, p. 290. Translated by F. H. Hatch, London, 1892.)

[3] See Oldham, David, Blanford, Feistmantel (3) and (6), Morris, etc.

Muree beds.—The marine strata containing typical Carboniferous fossil shells are of particular importance from the point of view of the correlation of Indian and Australian boulder deposits. The boulder beds of Bacchus Marsh in Victoria are also of the same geological age; they contain few plant remains, but a species of *Gangamopteris* has been recorded identical with one from the Talchir-Karharbári beds. From the Lower Coal Measures of New South Wales, that is the strata intercalated between the marine beds containing typical Carboniferous fossil shells, the following plants are known :—

Phyllotheca australis, McCoy.
Annularia australis, Feist.
Glossopteris Browniana, Brong., and three other species.
Nœggerathiopsis prisca, Feist.

In the Newcastle beds, which agree in age with the Damuda series of India, several species of plants have been found, *e.g.*:—

Phyllotheca australis, McCoy.
Vertebraria australis, McCoy.
Sphenopteris alata, Brong., and other species.
Glossopteris Browniana, Brong., and other species.
Gangamopteris augusti folia, McCoy.
Etc.

The Hawkesbury and Wianamatta beds need not be dealt with as they are referred to a higher horizon than that which more especially concerns us at present. The occurrence of rocks older than the Muree series is referred to later. In Queensland a series of strata occurs resting unconformably on Devonian Rocks; they have been classified as follows and referred to a Permo-Carboniferous age :—

Upper Bowen.
Middle Bowen.
Lower Bowen.
Star Series.
Gympie Series.

The flora of the two lowest series is "in no way allied to that which is found in the three succeeding divisions, but possesses a decidedly Carboniferous facies, the distinguishing plant being *Lepidodendron*".[1] From the Bowen River

[1] Jack and Etheridge, p. 70.

formations the *Glossopteris* flora has been recorded, including the following species :—

Phyllotheca australis, Brong. *Glossopteris Browniana*, Brong.
Sphenopteris lobi folia, Morris, *G. linearis*, McCoy. Etc.
and other species.

In Victoria a group of strata known as the Bacchus Marsh Sandstones rests on rocks containing Devonian and Lower Carboniferous plants, and includes extensive boulder beds of undoubted glacial origin. Among the plants recorded are :—

Gangamopteris obliqua, McCoy. *G. Spatulala*, McCoy. Etc.

In Tasmania[1] plant-bearing beds occur in the Mersey coalfield and in the neighbourhood of Hobart which are correlated with the Permo-Carboniferous beds of India and other regions. *Glossopteris*, *Gangamopteris*, *Phyllotheca* and other characteristic genera of the *Glossopteris* flora have been found in these strata.

A valuable addition has recently been made to our knowledge of Australian glacial deposits and rocks of Permo-Carboniferous age by Professor Edgeworth David.[2] He gives an able account of the stratigraphy of Australian districts and throws fresh light on the conditions which obtained during Permo-Carboniferous times.

Africa.[3]—Our knowledge of the South African beds is on the whole less satisfactory and more imperfect than in the case of India and Australia. An extensive area of the Cape is covered by a series of sandstones and shales with interbedded coal seams, known as the Karoo series. These rocks "occupy the largest portion of South Africa, as they compose the whole of the interior, forming the high elevated plains of the Kalahari, the Free States of the Transvaal, as well as the countries to the north as far up as the Limpopo; they are also to be met with at the Zambezi".[4] The Karoo formation has been subdivided as follows :—

[1] Johnston and Feistmantel (3), etc. [2] David.

[3] Oldham, Green, Moulle (good sections given in this paper), Waagen (1), Jones, Feistmantel (5), Schenck (a good geological Map of South Africa is given by Schenck), De Launay.

[4] Griesbach, p. 57.

Upper or Stormberg bed.
Middle or Beaufort beds.
Lower or Ecca and Koonap beds.

In the Ecca beds boulder deposits occur precisely similar to, and homotaxial with, those of India and Australia. The underlying Table Mountain Sandstones are often marked with deep grooves and exhibit other characteristics of ice-worn rocks. The following plants, among others, are recorded from this horizon :—

Glossopteris Browniana, Brong. *Nœggerathiopsis Hislopi*, Bunb.
Gangamopteris Cyclopteroides, Feist. Etc., etc.

From the overlying Beaufort beds we have a flora similar to that of the Indian Damudas and Australian Newcastle Strata.

In a collection of plants recently forwarded by Mr. Draper from a locality not far from Johannesberg, there are numerous *Glossopteris* leaves associated with casts of fairly well-preserved Lepidodendroid or Sigillarian stems. The association of these forms is of special interest, and a more complete examination of the specimens may throw fresh light on some points connected with the distribution of the Glossopteris flora. Professor Zeiller[1] has lately described several species, including *Glossopteris*, *Nœggerathiopsis*, and others from the neighbourhood of Johannesberg. It is impossible, however, in this summary to do more than mention the facts, but it is hoped to discuss elsewhere the nature of the South African plants in a more complete manner.

South America.—In 1889 Mr. Derby[2] recorded the occurrence in the Province of Parana of wide-spread boulder beds similar to those of India and elsewhere. His observations, however, were of a superficial character, and in themselves of little value for purposes of exact correlation. Recently some very important and interesting additions have been made to our knowledge of the geology of this country.[3] In 1869 Carruthers[4] described a number of plants

[1] Zeiller (6). [2] Waagen (2), letter from Derby.
[3] Blanford (4), Kurtz and Zeiller (4), De Lapparent and Steinmann.
[4] Carruthers (1).

brought by Mr. Plant from the Province of Rio Grande do Sol; Zeiller has now added to this list, and furnished additional facts of special value as regards the geographical distribution of Permo-Carboniferous plants. These Brazilian beds have now furnished the following species of plants :—

Psaronius brasiliensis, Brong.[1]
Lepidodendron Pedroanus, Carr.
Nœggerathia obovata, Carr.
Dadoxylon Pedroi, Zeill.
Odontopteris Plantiana, Carr.
Lepidophloios laricinus, Sternb.
Cordaites.
Gangamopteris cyclopteroides, Feist. Etc.

In this flora we have, therefore, plants of the same type as those characteristic of Northern Hemisphere Palæozoic rocks, associated with such a typical member of the *Glossopteris* flora as *Gangamopteris*. From plant-bearing beds in the Argentine Republic, Kurtz has recently recorded the following species :—

Neuropteridium validum, Feist.
Equisetites morenianus, Kurtz.
Nœggerathiopsis Hislopi, Bunb.
Gangamopteris cyclopteroides, Feist.
Sphenozamites multinervis, Kurtz.

Zeiller[2] correctly points out that Kurtz's *Sphenozamites* is probably a form of *Nœggerathiopsis*. He considers these Argentine plant beds are of the same age as those of Rio Grande do Sol, and places both on the same horizon as the Karharbári beds of India, the Ecca beds of Africa, and the Bacchus Marsh and Newcastle beds of Australia. Kurtz's paper has been translated and printed in the *Records of the India Geological Survey*,[3] and Blanford[4] gave an account of the recently discovered *Glossopteris* flora in Argentina in a short article printed in *Nature* in 1895.

The fossil plants described by Geinitz[5] and Szajnocha[6] from Argentina are from younger and older rocks, Rhætic and Culm, than those containing representatives of the *Glossopteris* flora.

In South Africa, India and Australia, undoubted glacial deposits occur in close association with strata containing

[1] Brongniart (2).
[2] Zeiller (4).
[3] Kurtz.
[4] Blanford (4).
[5] Geinitz.
[6] Szajnocha (1) and (2).

members of the *Glossopteris* flora. These beds are now referred on good evidence to a Permo-Carboniferous age. In South America similar boulder beds apparently occur, but as to these we possess but little accurate knowledge.[1] In describing the glacial beds of Australia, Edgeworth David writes: "In the case of Australia, Southern Africa and India, the general direction in which the ice moved appears to have been from South to North. In Australia the thickness of the glacial beds (unparalleled, so far as the author is aware, in any other part of the world, being about 2000 feet, if the intercalated beds of sandstone and conglomerate are included in the estimate) implies that the Permo-Carboniferous glacial epoch in the Southern Hemisphere was of prolonged duration."[2] From Bajo de Velis in the Argentine Republic typical species of the *Glossopteris* flora are recorded from beds which are in all probability homotaxial with those of Africa, Australia and India, and of Permo-Carboniferous age. From Brazil we have a flora of a rather different character; it consists in fact of such genera as *Lepidodendron* and others characteristic of European Carboniferous and Permian rocks, and unrepresented in the typical *Glossopteris* flora. Associated with these Northern Coal Measure genera *Gangamopteris* has been found, and this supplies a connecting link with the *Glossopteris* flora of Argentina and other districts. From New Zealand it would appear that no well-authenticated example of the genus *Glossopteris* has so far been recorded. In China, although Permo-Carboniferous rocks occupy a very considerable stretch of country, and have furnished abundant materials for a detailed monograph on fossil plants by Professor Schenk in Richthofen's *China*,[3] the *Glossopteris* flora has not been recognised in Palæozoic strata. The Palæozoic plants from the coalfields of China agree in the main with the typical Coal Measure forms of the Northern Hemi-

[1] Boulder beds have been described by Derby (*Records Ind. Geo. Surv.*, vol. xxii., pt. ii., p. 69, 1889) in Southern Brazil, and Blanford [Blanford (5)] considers that they are probably of the same age as the plant-bearing beds of Rio Grande do Sol.

[2] David, p. 300.

[3] Schenk.

sphere. It is of interest to note that Zeiller has recognised eleven species characteristic of the European Upper Coal Measures from Carboniferous beds in the coal basin of Tété in the Zambesi region.[1] We see, therefore, that European Coal Measure species are met with in a region far south of the typical Northern areas. From Tongking[2] *Glossopteris* and other genera of plants have been recorded, but the flora is probably of Rhætic age.

From the above lists of fossils and brief descriptions, it must be clear to any one who is but superficially acquainted with the typical Permo-Carboniferous plants of Europe and other parts of the Northern Hemisphere, that there is a striking contrast between the floras of this Geological Epoch in Northern and Southern latitudes. In the former we have *Lepidodendra, Sigillariæ, Calamites, Sphenophyllum, Pecopteris, Neuropteris, Sphenopteris*, and other well-known genera; in the latter these forms are for the most part absent, and in their place the *Glossopteris* flora is met with. On the other hand we have abundant evidence that several European genera of plants were in existence in Southern latitudes prior to the appearance of the Permo-Carboniferous *Glossopteris* flora. In an important memoir by Feistmantel[3] on Australian fossil plants, there are numerous figures of European forms from Lower Carboniferous and Devonian Rocks. From the Mount Wyatt beds of Queensland, or the Goonoo Goonoo beds of New South Wales and the Iguana Creek beds of Victoria, plants have been obtained indicative of an Upper Devonian or transitional horizon. In New South Wales[4] and Queensland plants occur which are referred to Lower Carboniferous European species. The following may serve as examples illustrating the occurrence in Australia of typical Carboniferous-Devonian species:—

Lepidodendron australe, McCoy.[5]
L. nothum, Ung.
L. Veltheimianum, Sternb.
Bothrodendron (*Cyclostigma*) *australe*, Feist.
Cordaites australis, McCoy.
Archæopteris Howitti, McCoy.
Rhacopteris intermedia, Feist.
R. Septemtrionalis, Feist.
Etc.

[1] Zeiller (2). [2] Zeiller (1). [3] Feistmantel (4). See also Etheridge (1).
[4] Ross, etc. [5] McCoy and Etheridge (3).

Several writers record species of *Lepidodendra*, *Stigmaria* and other familiar genera from strata underlying the Karoo Series of South Africa. Although many references to Lower Carboniferous plants occur in papers dealing with the Geology of the Cape, our exact knowledge of such plants is extremely small; the specimens are often spoken of as unsatisfactory, and no good figures have so far been published to enable us to form definite opinions as to the nature of the fossils.[1] In the British Museum and in the Museum of the Geological Society there are a few specimens of plants from South Africa which have been referred to different Coal Measure genera, but the material is extremely poor. Some of the supposed Carboniferous forms recorded from South Africa, for example those mentioned by Gray, are most probably specimens from the Coal Measures of Europe.[2] From Culm beds in Argentina Szajnocha[3] describes *Lepidodendra Pedroanum*, Carr., *L. cf. nothum*, Ung., *Rhacopteris cf. Machaneki*, Stur., *Cordaites cf. borassifolius*, Brong., and other forms. Zeiller considers that the plant referred to *Lepidodendron Pedroanum* is identical with the Culm species, *Lepidodendron Volkomannianum*, Sternb. In considering those Southern Hemisphere plants which agree with Culm and Upper Devonian species from Europe, it is important to bear in mind the fact that the *Lepidodendron* examples are in nearly every case partially decorticated stem casts, and cannot therefore be relied on with much confidence in the comparison of specific types. This danger has been pointed out by more than one writer, and Nathorst[4] has recently emphasised the need of caution in his valuable memoir on the Arctic Palæozoic flora. The plants from Bear Island, Spitzbergen, the Kiltorkan grits of Ireland, the Culm of Moravia and Silesia and other districts, afford instances of the striking similarity or even identity between the oldest known plants of Africa, South America and Australia and those from high northern latitudes. In India no plant-bearing beds are known older than the Lower Gondwanas.

[1] See Feistmantel (5), Gray, Green, Rubidge, Bain, etc.
[2] Feistmantel (4), p. 7. [3] Szajnocha (2). [4] Nathorst.

Summing up the facts in a few words, there are a few plant genera in Australia, South Africa and South America which agree closely with European Culm and Upper Devonian forms. Above this horizon in India, Australia and Africa the typical European plants are practically unrepresented, and in their place a new flora suddenly makes its appearance, characterised by *Glossopteris* and other genera of ferns and equisetaceous plants. In South America, and probably in South Africa, the European facies seems to have held its ground to a somewhat later period, *Lepidophloios* and other northern genera being found in association with representatives of the *Glossopteris* flora. It is a fact of considerable interest that many of the plants characteristic of the Permo-Carboniferous *Glossopteris* flora not only persist to a later epoch in the Southern Hemisphere, but also extend northwards and occur as fairly common species in European Jurassic beds. To mention a few examples, there is a close agreement between *Nœggerathiopsis Hislopi*, Bunb., and a leaf figured by Schoralhausen[1] from Russia as *Rhiptozamites;* the *Glossozamites* pinnæ figured by Feistmantel[2] from India fairly closely resemble similar leaves described by Kurr,[3] Schenk and others from European Jurassic and Wealden strata; the genus *Phyllotheca* is a well-known member of the Jurassic flora of Siberia, Italy, England and other countries; also another South Hemisphere equisetaceous plant, *Schizoneura*, occurs in the European Trias.

From some cause, then, it would appear that the Palæozoic vegetation, which in Lower Carboniferous times had a more or less world-wide distribution, was replaced in the South by a new set of plants, while in the Northern Hemisphere the older forms continued to flourish until the close of the Permian epoch, and were then superseded by a flora of a newer facies. The widespread glacial deposits of Australia, Africa, India and South America, point to conditions favourable to the existence of ice and a lowering of

[1] Schoralhausen, pl. xv., figs. 1-11.

[2] Feistmantel (1), pl. xx., figs. 4 and 5. [3] Kurr, pl. i., fig. 5.

temperature. There is, moreover, good evidence for the existence in Permo-Carboniferous times of a large continental mass of land stretching across from Southern Africa to India, and occupying part of what is now the Indian Ocean. In all probability South America also formed part of this Southern Continent, for which the name of Gondwana Land[1] has been proposed. Over this continent, which formed a land connection between Australia on the one side, and South America on the other, glacial conditions obtained of sufficient extent to give rise to the thick mass of boulder beds already alluded to. It is natural to suppose that there must have been a close connection between the lowering of temperature in Gondwana Land and the occurrence of the *Glossopteris* flora. We may, perhaps, assume that the Palæozoic plant types were unable to exist under the colder conditions, which on the other hand favoured the extension of a flora of a different facies, which may possibly have been derived from an antarctic continent. In speaking of the *Glossopteris* flora and the existence of Gondwana Land, Blanford remarks that perhaps the difference between the Upper Carboniferous flora of the Northern Hemisphere and the contemporary flora of the Southern Hemisphere, may be due to both isolation and climate. He goes on to say, "there is, moreover, some evidence in favour of the view that the transfer of the southern plants to the Northern Hemisphere was caused by a period of low temperature that drove a southern temperate flora northward to the equator".[2] "It is highly probable," he adds, "that many other forms of terrestrial life besides the Mesozoic flora originated in the Southern Hemisphere; and unless a very considerable area of what is now deep ocean was occupied by land in Mesozoic and Palæozoic times, a change in favour of which there appears but slight evidence, it is far from improbable that the antarctic continent was the original area of development." In a later paper the same

[1] Suess, vol. ii., p. 318, see also Blanford (5).

[2] Blanford (3), p. 105, see also Neumayr, vol. ii., p. 191.

author[1] writes: "The discovery in several different parts of South America of *Gangamopteris* and *Neuropteridium*, associated in the same beds with *Lepidodendron*, may perhaps imply that a land connection existed in newer Palæozoic times in the American area between the continent of the Northern Hemisphere and Gondwana Land". A consideration of the full significance of the recent discovery in South Africa of a lepidodendroid plant in association with *Glossopteris* is deferred until the material has been more fully investigated.

In dealing with an assemblage of extinct plants, it is extremely difficult, not to say impossible, to arrive at any definite conclusion as to the climatic conditions under which the different genera and species existed. We cannot adduce any trustworthy evidence from the plants themselves to warrant the assumption that the *Glossopteris* flora was well fitted to withstand a cold climate; but external evidence favours the conclusions as to climatic changes having been in a great measure responsible for the existence of two distinct botanical provinces during the Permo-Carboniferous era. Unfortunately we possess but little exact or complete botanical knowledge of the Southern Hemisphere plants; the work of Feistmantel and others has made us familiar with the various leaf forms characterised in many instances by their large size and anastomosing veins, but we are still without information as to internal structure and reproductive organs of the typical genera of the *Glossopteris* flora.

A more detailed and extended study of the Upper Palæozoic plants of the Southern Hemisphere, and a careful comparison of the homotaxial floras north and south of the equator, should lead to important generalisations interesting alike from the point of view of geographical botany, and the distribution of land and water at the close of the Palæozoic era.

[1] Blanford (5), p. 59.

	Australia.			Tasmania.	South Africa.	South America.		India.
	Victoria.	New South Wales.	Queensland.			Argentina.	Brazil.	
Permo-Carboniferous.	Bacchus Marsh. Sandstones. *Glacial beds.*	Muree beds. *Glacial beds.*	Bowen River Coalfield. *Glacial beds.*	Mersey Coalfield. *Glacial beds.*	Kimberley—Ecca beds. *Glacial beds.*	Ba jo de Velis Series.	Rio Grande do Sol. *Glacial beds.*	Karharbári. Talchiro *Glacial beds.*
Lower Carboniferous or Devonian.	Avon River. Sandstones. Iguana Creek beds.	Smith's Creek. *Lepidodendron* beds. Goonoo Goonoo Series.	Drummond Range, Mount Wyatt beds, etc.	Fingal Soft Shales. (No plants.)	Lower Carboniferous (*Lepidodendron*) beds and Zuurbergen—Zwartebergen and Wittebergen beds in Cape Colony. Devonian.	Retamito Series.		No plant-bearing beds.

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[The above list is very far from complete; several additional references may be found in the Bibliographies given by Feistmantel and other writers whose works are quoted above.]

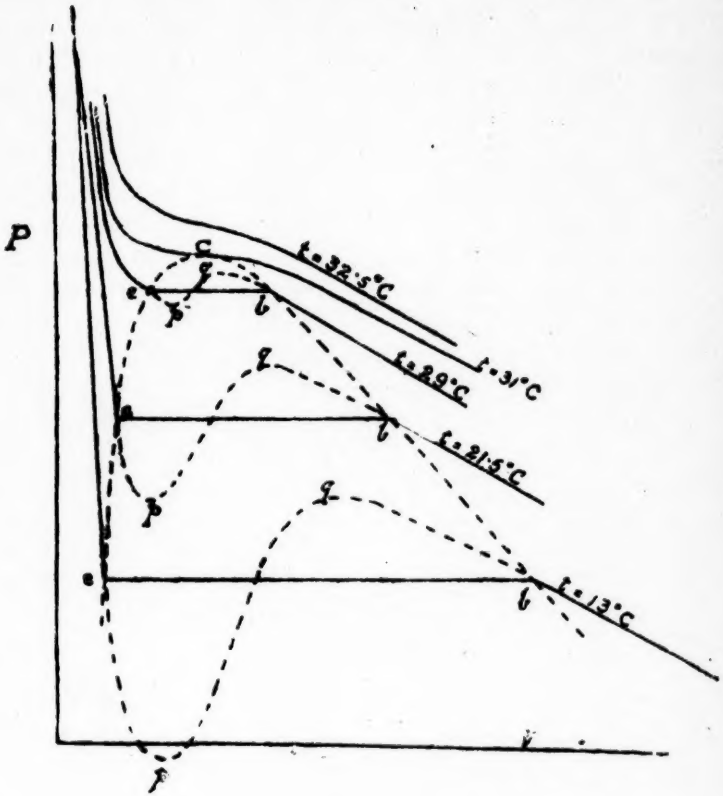
A. C. SEWARD.

CONDENSATION AND CRITICAL PHENOMENA.

I. SINGLE SUBSTANCES.

THE thirty years that have elapsed since the publication of Andrews' investigation of carbonic acid have made the dogma of "the continuity of the gaseous and liquid states of matter" common property. It seems worth while to review the history of that dogma up to the present time, both from an experimental and a theoretical point of view. The phenomena established by Andrews for carbonic acid consist in the occurrence of a so-called maximum vapour-pressure increasing with temperature, and of corresponding maximum vapour-density and minimum liquid-density, which approach each other the higher the temperature, and finally coincide in a point—the critical point. These facts are all exhibited in the diagram added to his paper, wherein p and v are the two co-ordinates. The diagram accompanying this paper gives an approximate idea of Andrews' diagram for carbonic acid; the meaning of the dotted curves added to it will appear later on. The curves drawn for constant temperature, the isothermals, show a horizontal part, representing the process of condensation. A curve is drawn through the points representing the volumes of the saturated vapour and liquid curve. This is called the border-curve, or the saturation-curve, and culminates at the critical point C. The isothermals of higher temperature have a continuous course, without the sudden breaks which the lower ones show where the process of condensation begins and ends, but still showing a deviation from the shape pv = constant, consisting in a flattening in the region of the critical point.

Since then a number of other substances have been investigated in the same or a similar manner. They all display qualitatively similar phenomena; the existence of a critical point has been established for almost all substances


p
c
t = 32·5°C
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that could be tested, and more or less accurate determinations have been made of their critical constants, especially of the critical temperature. But though the general features of the phenomena described by Andrews have been constantly confirmed by subsequent investigations, there is a considerable amount of uncertainty as to the details of the phenomena during condensation and near the critical point. It is maintained by a number of experimenters that neither agree fully with the simple conclusions to be drawn from Andrews' diagram. In the first place the existence of a constant vapour-pressure independent of the quantities of vapour and liquid present is denied. The fact is that even Andrews himself did not find a constant condensation-pressure, but one which increased slowly during compression and somewhat more rapidly near the end of the condensation. At 13·1° C. the pressure at the beginning of the process of condensation was 48·0 atmospheres, and at the end 49·5 ; at 21·5° C. the corresponding pressures were 58·9 and 61·3. But he ascribed this increase of pressure to the influence of a small admixture of permanent gas (1/500). Exactly similar phenomena, essential for mixtures, as we shall see in a second article, have been observed by a number of experimenters, and some of them are bold enough to assert that impurities were out of the question in their case at least, or try to prove that the way in which the pressure increased was inconsistent with the idea that this increase should be due to impurities. Even if we knew exactly how a mixture say of ether and an unknown second substance would behave when condensed, it would still be questionable if in the experiments the equilibrium between the two phases, liquid and vapour, was always properly established, for even small admixtures occasion very marked retardation in the establishment of equilibrium. One has only to think of the slowness of diffusion under any circumstances to understand what it may come to in a long and narrow high pressure tube. Moreover, we cannot help noticing that in a few cases experiments have been made with substances that did not show any increase of vapour-pressure at all. Young and Thomas prepared

isopentane of absolutely constant vapour-pressure; the same is true for carbonic acid as prepared and investigated by Amagat. This gas contained less than ·0004 of gaseous impurity. The writer of this article at one time prepared a sample of carbonic acid containing something like ·0007 of impurity, and which showed an increase of ·18 atmosphere at 15° C. Comparing this result with those of Amagat and Andrews for the same substance we come to the conclusion that for carbonic acid Andrews' explanation of the anomaly was right, and that it must be due to the presence of impurities, and if so in the case of carbonic acid, why not also in the other cases? At any rate to put it as mildly as possible, we are better not to give up the simple notion of a constant vapour-pressure until it has been more conclusively proved that pure substances display phenomena that one would expect to find in mixtures.

An explanation for the alleged behaviour of pure substances during condensation was readily found. If, say, ether behaves like a mixture when condensed, why not suppose it to contain two different kinds of molecules, vapour molecules and liquid molecules, the latter probably an agglomeration of the former? Both may be present in both phases, but presumably the liquid molecules will be much more numerous in the liquid than in the vapour. Would a mixture of that kind not show an increase of pressure during compression and condensation? An affirmative answer will be found in a sentence in Van der Waals' famous treatise on the continuity of the two states. But this is evidently a slip. The difference between a mixture of liquid and vapour molecules and a mixture of two substances is that in the former the molecules can change into one another. A simple application of thermodynamics, for instance of one of Gibbs' rules of phases, shows that the process of condensation will take place at constant pressure just the same as if the molecules did not associate. If this had been remembered by the promoters of the theory, it would have led to more caution in assuming the absolute trustworthiness of experiments.

The same idea of the existence of liquid molecules as

more complicated than vapour molecules (which, by the way, is a very reasonable idea in itself) is also constantly used to explain the abnormal phenomena near the critical point. "Normal" means here again agreeing with Andrews' diagram. In heating a substance at constant volume (say in a closed tube) the liquid surface ought to disappear only when the volume is equal to the critical volume; at greater volumes the liquid ought to evaporate, at smaller volumes the vapour ought to be entirely liquefied, in both cases before the critical temperature is reached. Neither the one nor the other is true; the numerous experiments on the subject agree in some points, but differ in quite as many. The liquid surface disappears and reappears at different volumes, and the temperature is sometimes found the same always, sometimes depending on the volume. Which volume and temperature is one to choose for the critical values? And even if we knew which, how is all this to be reconciled with Andrews' conceptions? And this is not all. A number of sometimes ingenious experiments, in U tubes, O tubes and otherwise, have been devised which seem to prove that even above the temperature of disappearance of the liquid surface there is a difference between denser and lighter substance. The liquid, though not separated from the vapour, would seem still to exist and only gradually to become mixed with the vapour. The point where the meniscus disappears would not be the true critical point, but the latter would be found at a higher temperature.

There is no doubt that observations near the critical point are of the utmost difficulty, the chief reason being the enormous compressibility and expansibility of the substances in that region. It has also been proved that near the critical point exceedingly small admixtures have an enormous influence on the density. In an experiment due to Galitzine two quantities of ether, separated by a column of mercury, are enclosed in a U tube. The tube is heated above the critical temperature, and shows differences of density in the two limbs. According to Galitzine, these are due to a greater number of liquid molecules on the one side of the U tube. The writer of this article repeated this experi-

ment. The ether was purified as much as possible, but in the end the tubes appeared to contain minute quantities of gas which it is very difficult to get rid of. In one of his tubes they were estimated at ·000086 and ·000026 in the two limbs respectively (in molecules to molecules of ether). Differences as observed by Galitzine, but smaller, appeared to exist. At 195° C. (critical temperature, 194·3°) the difference amounted to 4·5 per cent. In a second experiment a portion of the permanent gas was transported from the one limb to the other, which now appeared to contain ·000029 and ·000297 respectively. Again, a difference in density existed above the critical point at 195° C. amounting to 19 per cent., but *in the opposite direction;* while the difference in pressure was 17·5 mm. mercury in the first case and 13·1 in the second. The difference of 4 mm. is by no means sufficient to account for the difference in density, which must have been for the greater part due to the small admixture of gas which was transported from the one limb into the other. It is obvious that in an observation of the critical point in a straight tube a similar difference of density may occur, as the liquid and the gas contain different percentages of the impurity. As the process of diffusion is a very slow one, one would expect these differences to continue even though the temperature be raised above the critical point, and the phenomena that one would predict this way have a remarkable resemblance to a number of the phenomena actually observed. If the difference in density were due to a difference in the number of liquid molecules, one would expect these differences to be almost instantly equalised by the exchange and rearrangement of the particles, as this process involves no diffusion, only dissociation and association of identical particles. The presence of impurities would go a long way in explaining also the other abnormalities described above, but there is another important factor to be taken into account—*viz.*, the influence of gravitation. This influence had been overlooked until Gouy drew attention to it. It makes the density in the tube different at different levels. The density in one point may be the critical density, while in points above or below it the

density is smaller or higher. The result is that the liquid surface may disappear at any height in the tube, depending on the total volume occupied by the substance, and the critical phenomenon will not take place at one volume only, though always at the same temperature. This fact added to the influence of impurities seems to me to explain all abnormal phenomena observed, in so far as uncertain and often contradictory phenomena of that kind can be explained. The theory of the association of molecules in the liquid state seems superfluous and inadequate to explain the abnormalities.

From the above it would appear that pure observations are almost impossible to make at the critical point. In studying the more recent literature on the subject it is found, however, that the more trouble is taken to obtain pure substances the more normal the critical phenomena come out. This is, for instance, so for an interesting experiment devised by Cailletet and Colardeau. In an O tube the under half of which is filled with mercury, two unequal quantities of liquid carbonic acid are condensed in the two limbs. The mercury naturally shows a difference in level; in heating the tube to and above the critical temperature the difference does not disappear at once, and cooling the tube again gives more liquid in the limb where there was an excess of liquid before. This experiment has been repeated probably with purer gas, and the difference disappeared only quite a little above 31°, and equal quantities condensed at the two sides in cooling. The presence of some impurity in the liquid or gaseous CO_2 explains the former result readily. Even in cases where impurities have not been entirely removed (and that would be in 99 out of 100) very trustworthy experiments may be obtained by thoroughly stirring the substance. A little iron rod moved up and down in the tube by a strong electro-magnet outside serves the purpose admirably and is astonishingly effective. The equilibrium between the two phases, which, according to some very interesting time experiments of Gouy, would take days to establish itself, only takes a few moments when the stirrer is set in action; a result at once

explicable on the impurity-diffusion theory developed above. The critical phenomena become very simple, and in accordance with theory, even for gases in which the presence of some impurity is revealed by a not quite constant vapour-pressure. The temperature at which the liquid-surface disappears is found independent of the volume and is therefore easily ascertained; the best method is to apply a slowly rising and falling temperature alternately. If we assume the critical isothermal between certain limits to have a similar shape on both sides of the critical point, the true critical density will be found by calculating the average density when the liquid surface disappears half way up the column. Very concordant values for the critical density are found by this method, and I believe that the optical method (if thus applied) does not deserve all the severe things that have been said about it. There is only one point that leaves some uncertainty in the method; one or two tenths of a degree above the critical temperature a very characteristic blue mist fills the tube, probably due to liquid particles floating in the substance. But in his best experiments the writer of this article saw this mist disappear at less than ·2 above the critical temperature as measured by the disappearing of the liquid surface, and the uncertainty is therefore very small indeed. If no sudden changes of volume or temperature take place all the other critical phenomena, like the striæ described by Andrews, hardly show themselves, if they do so at all.

There are other methods for determining the critical constants. Chappuis, for instance, measured the refractive index of the two phases by an interference method, and calls critical temperature the temperature where the two coincide. But if impurities are present, and retardation in the process of mixing, the refractive indices will not be equalised at the critical temperature, and the method is bound to give a result which is too high. For carbonic acid he found 31·6° C. (Andrews 30·90°, Amagat 31·35°). Amagat determined the critical temperature of CO_2 by plotting the densities as far as possible and completing the curve. A direct method would seem preferable, though in the hands

of an experimenter like Amagat the method no doubt yielded a very accurate value. The direct method described above gave the writer of this article 31·2° C. for CO_2 containing about ·0007, a very small difference to be sure.

The critical volume is now generally obtained by a graphical method depending on a law discovered by Cailletet and Matthias, that "the mean of the vapour and liquid densities is a linear function of the temperature". Where this line cuts the border-line of densities the critical density is found. The law holds very well where it has been tested (for instance, for CO_2, Amagat; isopentane, Young and Thomas, etc.). [Certain exceptions will be referred to in the sequel.] From a minute study of the best results obtained, one would conclude that the line is not perfectly straight, and therefore the law only approximate after all. This is of importance because it suggests the question in how far one is entitled to use it for determining the critical density. There are other cases in which empirical laws which hold perfectly at some distance from the critical point begin to fail near the critical point. The height of a liquid in a capillary tube is a linear function of the temperature, until close by the critical temperature, when a marked curvature in the line appears. It is, however, questionable if the determination of the critical density (even if made according to the principles laid down above) will ever be accurate enough to discover a small deviation in the "diameter" near the critical point, if there is any. But it will have to be tried. There are certain groups of bodies which do not obey the law of the diameter, but yield a line with very marked curvature. These bodies, such as the fatty acids, the normal alcohols, water, curiously enough are the same as disobey most of the other laws that hold for other substances. This exceptional behaviour is ascribed to a conglomeration of molecules to groups chiefly at the higher densities; a similar association of molecules would not take place for the majority of substances which follow the general laws.[1] A very satisfactory justification of this explanation

[1] Compare an article by Dr. J. Shields in "SCIENCE PROGRESS," pp. 265-278, December, 1894.

will be discussed presently, and the theory of the association of molecules to groups for those substances will then appear a very probable one. But it must once more be pointed out that an association of this kind would never be able to explain a change in the vapour-pressure, as has been attempted by Batelli and others. This was already discussed above.

The problem of the behaviour of molecules in the different states of matter belongs to the kinetic theory of matter, a theory which had been worked out for gases long before Andrews published his investigation on the condensation and critical phenomena of CO_2. It was Van der Waals who succeeded in drawing Andrews' results within the range of the kinetic theory, and at the same time expanded the idea of the continuity of the two states. It was suggested by James Thomson that the isothermal does not really possess the two sharp breaks at the beginning and at the end of the condensation, but that the two parts of the curve outside the border-curve are to be joined by a curve with a maximum and a minimum like a wave-crest and a wave-trough. Three of such curves are given in the diagram. The reality of the phases between e and p is shown by the possibility of lowering the pressure of a liquid a good deal below its vapour-pressure as long as no vapour is present. The phases on the crest part (b 9) have not been realised in the same way. But both these and the phases between e and p occur where liquid and vapour are separated by a curved surface. All this makes the existence of the rest of the hypothetical curve more or less probable also. As early as 1873, Van der Waals published his famous memoir in which he proves that the kinetic theory leads to an equation for the isothermal having the shape proposed by Thomson. The equation is $\left(p + \frac{a}{v^2}\right)(v - b) = R\,(1 + \alpha\, t)$. This result he obtains by assigning a finite size to the molecules and attractive forces between them acting over distances large compared with the distances of neighbouring molecules. The term in b depends on the former, the term in a on the latter supposition. By increasing t in the formula

the wave-shape of the curve gradually disappears, and above a certain temperature has vanished altogether. The point in which the wave disappears as it were has all the properties of Andrews' critical point. In fact qualitatively Andrews and Van der Waals' diagrams are exactly alike, at least outside the border-curve. Inside the curve the theoretical isothermal has the wave shape as described with points between p and q in which the condition of the substance is essentially unstable. $\left(\frac{dp}{dv} > o\right)$ Experiment shows that the substance divides itself into two portions, both stable and also in equilibrium with each other, an equilibrium which is independent of the qualities of the two "phases" present, the pressure depending on the temperature only. The kinetic theory is not able to determine, in a general way, the condition of equilibrium, and in that way find points belonging to the border-curve. Too little is known of the nature of the superficial layer between liquid and vapour. In cases of this kind we take refuge in thermodynamics. The criterion to which it leads in this case was found by Maxwell, and afterwards by Clausius, and may be stated thus: the horizontal line which connects the two co-existing phases on the isothermal encloses with the wave-crest and the wave-trough surfaces of equal area. By this rule it is possible to determine the vapour-pressure, and the vapour and liquid density from the theoretical isothermal. But no more than other rules arrived at by thermodynamical considerations does it give us an insight into the mechanism on which the equilibrium of the two phases depends. Kinetically the condition of equilibrium is this: that the same number of molecules passes from one phase into the other, as backwards; but, as was said before, this condition cannot be applied unless by making special assumptions about the nature of the bounding surface, which impairs the generality of the result obtained.

Quite lately, a most interesting theory was published by Prof. Van der Waals regarding capillary phenomena, in which he imagines the surface layer between the phases to contain all the phases on the theoretical isothermal between

the liquid and vapour, in the order in which they lie on the isothermal. It is impossible to explain here how the condition of equilibrium is arrived at. Suffice it to say that the theory is thermodynamical. Though among these phases there is a group of phases necessarily unstable when supposed to exist over a finite space, Van der Waals proves that the layer as conceived by him, in which each of the phases exists over an infinitely small distance, is stable as a whole. According to this theory the continuity of the two states within the border-curve would not only be an imaginary, a theoretical one, but a real one, and in a seemingly discontinuous surface between two phases we should have a special kind of continuous transition. This is the widest possible generalisation of the dogma of the continuity of the two states. The more one considers the matter the more one is struck by the beauty of the theory, and the remarkable simplicity of the conception of the continuity which it contains.

The application of the theory to a curved surface leads to some interesting results. The condensation-pressure appears to be different in that case to what it is at a plane surface, a fact which a direct application of thermodynamics had already revealed. The pressure is higher in one of the phases than in the other, and both differ from the ordinary condensation-pressure given by the height of the straight lines in Andrews' diagram, being either both higher or both lower.

It is also found that possibly a continuous transition exists above a certain temperature only, below which the transition would have to be discontinuous; and a number of other things as well. The theory leads to a law giving the change of the capillary constant σ with temperature t near the critical temperature t_c, *viz.*, $\sigma = c\left(1 - \frac{t}{t_c}\right)^m$ in which c is a constant and m would be $1\frac{1}{2}$ close to the critical point. This law was tested with the experimental results obtained by De Vries and Ramsay and Shields; it appears that at some distance from the critical temperature m is a constant quantity, but smaller than $1\frac{1}{2}$, about 1·25 for most substances. The only exceptions are the same substances

that were mentioned before as disobeying the law of the diameter, and for which an association of molecules is assumed. The capillary constants obtained for these bodies allow of a calculation of the amount of association by means of the law of corresponding states, a law which will be considered later on. This will be explained in Dr. Shields' article quoted above.

The law could not be tested up to the critical point as the experiments did not go far enough. Verschaffelt has measured the constant for carbonic acid up to 30° C. and finds that m is higher than 1·25 near the critical point. But his results depend greatly on Amagat's values for the densities of CO_2 near the critical point, which at temperatures near the critical point were found by graphical interpolation from observations at lower temperatures; this method unhappily leaves uncertainties which make a decision between 1·5 and, say, 1·3 impossible. Suppose, however, the law $\sigma = c\left(1 - \frac{t}{t_c}\right)^{1\cdot5}$ was not confirmed, this would not show that the idea of continuity in the surface-layer ought to be given up. The way in which the formula is arrived at seems to show that it depends on Van der Waals' equation of condition mentioned before, which, though of great importance, has been shown not to agree numerically with observed facts.

The difference between the formula $\left(p + \frac{a}{v^2}\right)(v - b) = \mathrm{R}\,t$ and observation is too absolute to be treated as a mere correction. In fact, though at the time it was impossible to say in how far the formula would agree with facts, one thing was pointed out by Van der Waals himself, *viz.*, that the formula would certainly not hold beyond certain densities. Its applicability has since been proved to be even smaller than was anticipated and might have been expected after the successful calculations with regard to carbonic acid. For one thing, it leads to a value for the critical volume $v_c = 3\ b = (\text{approximately})\ \frac{3\ t_c}{8 + 273\ p_c}$ which relation has never been confirmed, v_c always being a great deal less than

its value, calculated from p_c and t_c, mostly about $\frac{2}{3}$. Again, the critical volume should be 2·667 times less than the volume calculated according to Boyle's law, while it has been found to be 3·7 or 3·8 times smaller. Apparently even at the critical point Van der Waals' equation does not hold. It seems as if the changes in density on the isothermal are much more sudden than according to Van der Waals' formula, and as if the process of transition from vapour to liquid is connected with a more intimate combination of the particles. Attempts have been made to improve upon Van der Waals' formula by starting from more general suppositions about the molecular actions, both at a distance and during collision. Formulæ with more than two constants are obtained that way which can be made to agree with facts over a larger area of the diagram. Those formulæ are mostly half empirical and are nothing more than Van der Waals' formula with a few more judiciously introduced constants. Professor Tait arrives at a formula containing four constants, the physical meaning of which is well defined; but even this formula does not yield satisfactory results. The value of the critical constants is derived from the formula in the way originated by Van der Waals; but in order to make the formula agree with Amagat's results for carbonic acid it appears necessary to calculate two systems of constants based on two different values for the critical volume, one set for the observations at higher and the other set for observations at lower density. Even then the agreement is not perfect. Professor Tait justifies this doubling of his constants by the fact that experiments show the critical volume to lie between two limits. But it was shown above what this apparent range of critical volumes was due to, *viz.*, the action of gravitation. At the same time, even if the critical isothermal was very nearly horizontal and straight in the critical point, how can v_c, the volume in the summit of the border-curve, by the way in which it is derived from the formula possibly have two values or a range of values? It seems, therefore, that even Professor Tait's formula with its four constants is not capable of describing the behaviour of carbonic acid. And

though one or other of the formulæ mentioned may occasionally be used with success, the problem of a satisfactory formula does not seem much nearer its solution than where it was left by Van der Waals in 1873. There is one law, however, essentially empirical, which has been found pretty well confirmed where tested, *viz.*, that the pressure at constant volume is a linear function of the temperature. The accurate series of observations due to Amagat, Ramsay and Young, etc., confirm this law, though the most recent results obtained for isopentane and hexane by Young and Thomas, and for a number of liquids by Barus and especially Amagat, show slight but certain deviations. Van der Waals' formula is in form a special case of this law, but theoretically there is nothing to say in favour of the general one.

While it is at present impossible to explain the behaviour of a substance in detail on the kinetic theory, a remarkable set of laws has been discovered which shows the existence of a great resemblance between the behaviour of different substances, the laws of corresponding states of matter. The law was deduced by Van der Waals in 1880 from his formula, but appears to hold in circumstances where the formula itself would not be applicable at all. This is quite intelligible, as the law would follow from a good many equations different from Van der Waals' equation. In fact, as was first pointed out by Kamerlingh Onnes in 1881, it follows from a general hypothesis, *viz.*, that the molecules of different substances are similar bodies of constant dimensions acting upon each other with attractive forces that can be reduced to a surface pressure, or forces inversely proportional to a certain power of the molecules. In both cases by the application of Newton's principle of the dynamical similarity the law of corresponding states of matter is arrived at. The simplest way in which to put the law is that if p, v and t are expressed in terms of their values at the critical point, the relation between them is the same for all substances. This law leads to numerous consequences which may be used in testing it, a few important ones of which I will here mention. In the first place, that there must be a relation between the three

critical constants, so that say v_c may be found from p_c and t_c; also that v_c should be in a constant ratio to the volume as calculated by the laws of Boyle and Charles. These two relations were mentioned above, and we saw how they are confirmed by experiment. Another consequence of the law is that the vapour-pressure expressed in terms of the critical presence will be the same for all substances at temperatures which are the same fractions of the critical temperature. Similar relations regarding the densities of vapour and liquid, and even quantities like the capillary constants, co-efficients of viscosity, co-efficients of expansion, etc., may be derived from the general law. It is difficult within the scope of this article to give an idea in how far experiment confirms these laws. Sometimes the agreement is very close, in others it is less satisfactory, but for most substances at least approximate even for pretty high densities. Again, the acids, alcohols, water, etc., the groups of bodies that were mentioned before as disobeying other laws of liquids, form exceptions. It will be better understood now what right we have to explain the abnormal behaviour of those bodies by association of molecules. If we assume that for the majority of substances no such association takes place, substances in which it does cannot be regarded as dynamically similar, and for them the law cannot therefore hold.

These laws in so far as they are confirmed restrict the number of isothermal equations that are possible. When in the isothermal equation we introduce the critical constants as far as possible, all constants like a and b in Van der Waals' equation, in other words, everything specific for the special substance, should disappear. An equation in which that does not happen is in contradiction to the law of corresponding states. On the other hand the hypothesis on which the law may be based shows that there are quite a number of equations to be imagined which could be in agreement with the law.

Finally, therefore, we are confronted with the fact, that though important laws both empirical and semi-theoretical have been discovered, a satisfactory equation between p, v, T

does not exist. Perhaps the problem to find one which would embrace the whole field of possible values for p, v, T is insoluble, but the dogma of the continuity as founded by Andrews and enlarged by Van der Waals seems independent of this equation. If we take Andrews' diagram and complete it inside the border-curve in the manner of Thomson and Van der Waals, we have the whole dogma before us. The conditions outside the border-curve are to be realised by experiment, those inside only under special conditions or in the surface layer between the two phases. The reality of the isothermal inside the border-curve is hypothetical, but so acceptable that at present in similar cases of co-existence of phases the existence of a theoretical continuous transition between the phases is invariably admitted. We might almost say that thermodynamics is essentially dependent on hypotheses of that kind.

In the light of the dogma it is easily seen that a difference between liquid and vapour or gas does not exist. In fact this point was made entirely clear by Andrews himself. It is, however, not unnecessary to lay some stress on this point, as even now there seems to be a tendency in the minds of physicists to define gas-vapour and liquid in a way that shows that the bearing of the continuity is not sufficiently understood. Very generally one finds a substance called a gas (or a true gas) above the critical temperature and vapour (or liquid) below the critical temperature. This will do as a conventional distinction, but there is no real foundation for it. One might just as well call a substance a gas above the critical pressure and a vapour (or liquid) beneath it. The critical constants have a special meaning *in the critical point only*, and the critical isothermal is no line of demarcation between different conditions or properties of the substance.

In a second article the condensation and critical phenomena of mixtures will be considered, to which we have been led up naturally in discussing the experiments on so-called pure substances.

J. P. KUENEN.

THE ORIGIN OF LAKES.

THE study of lakes has recently attracted so much attention that Limnology is now recognised as a special branch of geography, and a large number of problems connected with lakes are being solved. The one which has been of the greatest interest to geologists is without doubt that of the origin of the hollows in which lakes are contained, and although the existence of a large number of these hollows has been satisfactorily explained, there are many others whose origin is still uncertain. As the subject is one which may well occupy the attention of local observers, no apology is needed for attempting to show the position in which we now stand as regards our knowledge of the origin of lakes.

General interest in the question of the formation of lake basins was undoubtedly aroused in the first place as the result of the late Sir Andrew Ramsay's paper "On the Glacial Origin of Certain Lakes in Switzerland," etc., published in 1862(1), in which he ably advocated the formation of rock basins by glacial erosion, but his views are so well known that it is only necessary to allude to this paper as the starting-point of modern work on lake formation. Since that time a host of observers have paid attention to the physical structure of lake basins, and we now have accurate surveys of a large number of lakes, notably those made by Mill in England and by Delabecque in France. As the result of this work, the subaqueous scenery of lakes is in many cases as well known to us as that above water, and we are consequently able to apply our knowledge of the mode of operation of different agents of erosion with greater certainty than could be done before the execution of these surveys.

Though the term "rock" is applied by geologists in most cases to aggregates of mineral without reference to the compactness or coherence of the component particles, its

use in a somewhat different sense in the case of lakes is not likely to give rise to any confusion, and there is no objection to adopting the usual classification of lakes into two divisions, namely, the lakes which are contained in rock basins, and those which are not, though perhaps a four-fold division would be more useful, the lakes being classified according as they occur (i.) in craters, (ii.) in depressions which have been blocked up by some material, (iii.) in areas which have been depressed by earth movement, and (iv.) in hollows of erosion; this latter classification I propose to use in the present paper.[1]

(i.) *Crater lakes.*—The existence of lakes occupying the site of volcanic craters has long been known, and their origin is quite apparent. Many crater-shaped hollows are ready to receive water, after the cessation of volcanic action; the Lucrine Lake and Avernus are classical examples of lakes formed in this manner. Several crater lakes of the volcanic region of Central France have been sounded by M. André Delabecque, and their symmetrical shape and considerable depth shown (2). Lac d'Issarlès in the department of Ardèche has a depth of 108 metres, Lac Pavin of 92 metres, Lac de Tazanat of 66 metres, and Lac Chauvet of 63 metres. The three last-mentioned lakes are in the department of Puy de Dome, and their boundaries and subaqueous contours are remarkably circular. Concerning Lac d'Issarlès, Scrope writes that it is one of those lakes which "differ from ordinary craters, not only in their greater dimensions, but in the nature also and disposition of their enclosure, which is usually of primary, or, at all events, pre-existing rocks, merely sprinkled more or less copiously with scoriæ and puzzolana, little if at all elevated above the surface of the surrounding country"; it thus becomes clear that crater lakes may lie in hollows formed by explosion or by accumulation of volcanic ejectamenta, or by a combination of the two processes.

[1] I had written this article before reading the admirable chapter upon lakes in the second edition of Sir John Lubbock's *Scenery of Switzerland*, where a similar classification is proposed. I hope all who are interested in the question of lake-formation will refer to that work.

(ii.) *Lakes in depressions which have been blocked by some material.*—The variety of materials which by damming up valleys give rise to lakes is very great, and a much larger proportion of lakes than is commonly supposed probably owe their existence to the formation of a dam across a valley. In the case of such blocked valleys, the water which accumulates behind the dam to form a lake may eventually find its way over the dam, and if this be composed of incoherent material, as frequently occurs, the dam is readily worn away and the lake is a short-lived one and is rapidly drained, its position being often marked by a peat-bog, overlying the deposits which were laid down on its floor prior to its destruction. The number of peat-bogs occurring behind dams, as compared with existing lakes in such a region as that of the English Lake District, proves that the surviving lakes are a mere fraction of those which once existed. But other dammed-up valleys are blocked by dams, of which the lowest point is at a higher level than that of a col uniting the valley with an adjoining one, in which case the lake fills up to the level of this col and drains through it, oftentimes over solid rock; lakes of this character are much more durable than those which discharge over incoherent dams, and are likely to survive when the latter are destroyed. I have elsewhere attempted to prove that a large number of the tarns and lakes of English Lakeland were formed in this way, and that their formation was coincident with deviations of the drainage system, often of considerable importance (3).

Passing on to consider the various kinds of dam which give rise to lakes, we may notice in the first place lakes held up by ice, whether in the form of an ice avalanche or a glacier. The lakes formed by ice avalanches are necessarily of an ephemeral character and are mainly interesting on account of the damage wrought by floods when they burst. The best known instance of a lake of this character is that of the sheet of water produced by the blocking up of the Dranse by the Gétroz Glacier, which caused the disastrous flood in Rhone Valley in 1818. Of glacier-dammed lakes, the Märjelen See is too well known to require more

than passing notice; but magnificent examples occur in Greenland, and in some of these the water drains over or under the ice, whilst in others the outflow resembles that from the Swiss lake in being over solid rock. Some interesting lakes of this character are figured in Plates A and C of the first part of the *Meddelelser om Grönland.* I need only refer here to the famous Parallel Roads of Glenroy and the great terraces of "Lake Agassiz" in North America, which are considered to be due to glacial dams of this nature.

In the lowland tracts of river-basins crescentic lakes often result from the formation of "cuts-off," the former sinuosities of the river being barred by river-deposit and giving rise to lakes. Good examples of these are found in the lower reaches of the Mississippi and are described in the nineteenth chapter of Lyell's *Principles.* Accumulations of vegetable matter are frequently sufficient to obstruct the drainage of streams to so great an extent as to form lakes, which are subsequently converted into peat-mosses. Sir A. Geikie records the formation of lakes in North America which have covered thousands of acres and which were due to the formation of a dam of vegetable matter produced by beavers cutting down trees.

Blown sand is another material which is competent to form a barrier enclosing a large sheet of water. The well-known *étangs* in the neighbourhood of Arcachon were formed by the gradual advance of sand-dunes from the ocean, converting bays into inland pools, the surfaces of which are frequently at a considerable height above the ocean level.

In upland districts numerous tarns and lakes are blocked up by barriers formed of landslip material, screes, dry deltas, snow-slope detritus and glacial accumulations. A good example of a lake formed by landslips is that produced near Terranuova during the Calabrian earthquake of 1783. Examples of the other barriers are furnished by various tarns in the Lake District. Goatswater, near Coniston, is blocked by screes; Smallwater, near Haweswater, by snow-slope detritus; several of the tarns by glacial accumulations,

especially the moraines of combe-glaciers, whilst Sty Head Tarn is a good example of a lakelet due to the formation of a dry delta, formed by a stream descending from the two Gables. The freshness of the surface of one bank of the stream, where much material has evidently fallen in, indicates that the formation of this tarn may have been quite a recent event, and it would be interesting to know whether there is any proof of its non-existence in historic times. It is evidently becoming filled up very rapidly by the deltas of the streams which flow into it, and this also indicates a recent origin. Although several of the lakeland tarns have exits flowing over the barrier, the larger proportion of them drain over solid rock, and naturally several of these occur amongst the shallow valleys of the upland plateaux where cols lower than the drift-barriers frequently existed. The tarn at Watendlath, if drift-dammed, requires the filling in of a very deep cañon-like gorge, whose depth must have been much greater than its width at the top; if the evidence for the formation of the other tarns of Lakeland by drift-dams were not so strong, one would feel inclined to admit that Watendlath Tarn lay in a true rock-basin, but it is hardly likely that this tarn had a special origin, and after re-examining the surroundings I am persuaded that the drift-filled gorge did exist. This would necessitate the beheading of the old Lowdore-Watendlath Valley in pre-glacial times by a ravine cutting back from Rosthwaite which became filled with drift in the glacial period. It is very desirable that some similar case in a less picturesque area should be investigated by boring, for if narrow ravines were common in pre-glacial times, a great many apparent rock-basins may have narrow, sinuous, drift-filled gorges, which might easily escape detection. The comparative absence of narrow gorges in our upland regions is a somewhat surprising fact, and I am inclined to think that the true explanation is that a large number of them, which existed in preglacial times, have been filled up and completely concealed by drift accumulations. Under the heading of lakes in depressions which have been blocked by some material may be included those lakelets which lie

solely in hollows in drift, the barrier in this case extending all around the lake. The name "kettle-holes" has been applied to lakelets of this character by some American geologists, but the late Dr. Carvill Lewis, who gave a good account of them, spoke of them as morainic meres. Good examples occur "at Ellesmere in Cheshire. At this picturesque locality most of the meres—over twenty-five in number—have received separate names all ending in 'mere'" (4).

Turning now to larger lakes, which are due to barriers of drift, I have given reasons for believing that the principal lakes of English Lakeland are also due to the blocking of valleys by barriers of drift, and that they do not occur in rock-basins. These lakes have frequently been sounded, most of them in considerable detail by the late Mr. Clifton Ward whose work on the Lake District was one of the most substantial contributions to the geology of that region (5), but especially by Dr. H. R. Mill (6) whose valuable paper has given us a true insight into the character of the subaqueous scenery of these lakes. It is clear, from perusal of Dr. Mill's work, that the subaqueous features of the lakes do not differ in any essential particular from the subaerial features of the adjoining valleys, and that subaqueous and subaerial features alike have been produced by the same agents of erosion; the character of the scenery in each case forbids the supposition that it has been produced by glacial erosion, and we find plenty of evidence that glacial erosion has merely given a finishing touch to the work of the other subaerial agents. We must, therefore, attribute the formation of these lakes to earth-movement or blocking of valleys by drift accumulations, and I have given reasons, after study of the distribution of the drift and of the anomalous courses of many of the rivers of Lakeland, for attributing the formation of the lakes to the latter cause. Some of the lakes, such as Ullswater and Haweswater, appear to drain over the drift-barrier, but the greater number, including Windermere, Bassenthwaite, Thirlmere and others, have the drift-filled valleys occupied by insignificant runnels,

whilst the lake is drained through a rock gorge, whose head once formed a col at a lower level than that of the drift barrier. A similar instance of the formation of lakes, accompanied by a marked deflection of drainage, has been described by M. Delabecque in the case of Lakes Longemer and Gérardmer in the Vosges district (7). This method of formation of lakes was described in 1894 by the late Professor Carvill Lewis, in the work already cited. He states that "if the moraine is compact, an outlet for the lake is often cut through the rock, and a post-glacial gorge is formed". He gives Lake Constance as an example, and observes that "this lake is dammed on its north side by the great terminal moraine of the Rhine glacier, and owes its large size to the flatness of the region in which it lies. Its present outlet to the west by way of Schaffhausen, is a post-glacial one cut through rock. Before it was obstructed by the moraine, the pre-glacial Rhine flowed northward into the Danube." Lakes blocked by moraines, whether the outlet is now over the moraine or over solid rock, are spoken of by Professor Carvill Lewis as "inter-morainic lakes".

Before quitting the class of lakes produced by the formation of a dam across a valley, allusion must be made to those due to a lava-flow obstructing the drainage of a valley, as happened in the case of the lava-flow descending from Etna in 1603, which blocked the valley of the Simeto. Lakes must frequently have been produced in this manner in volcanic regions.

(iii.) *Lakes in areas which have been depressed by earth-movement.*—Differential movement of portions of the earth's crust has long been known to have produced lake-basins both in volcanic and non-volcanic regions. A good example of a lake of this character in an old volcanic region is Lake Balaton (the Platten See) in Hungary, which has been described by Professor Judd (8), and the instances described by Dr. Gregory along the "Great Rift Valley" of Africa are now familiar to all readers (9). The proofs of the formation of lakes by earth-movement are particularly clear in the case of the ancient lakes Bonneville and Lahon-

tan in the western territories of North America, for the deformation of the terraces has been worked out in the case of those lakes, and some of the terraces are actually seen to be cut by fault planes (10). Dr. Gilbert, who described the geology of Lake Bonneville, has also detected proofs of differential movement amongst the old terraces of Lake Ontario, and a number of papers have been published by Dr. J. W. Spencer, giving accounts of his detailed work on the "warping" or deformation of the terraces around several of the large Canadian lakes of the St. Lawrence Valley, one of which appears in the *Journal of the London Geological Society* (11). It may be observed here, that although there is evidence of considerable earth-movement having affected the British area since the formation of the principal British lakes, it is doubtful whether any of these lakes owe their origin wholly or in part to this earth-movement, which appears to have been singularly uniform in amount over very wide areas, and therefore to differ from the "warping" movements which have taken place in Canada and the Western Territories. Still it is possible that differential movement has played some part in the formation of some of our lakes, but it will require much more work upon the physical structure of our lake-basins before the partial or entire production of any of them by differential movement can be established or disproved.

There is one kind of earth-movement due to local causes which must be noticed, as some lake-basins are undoubtedly produced thereby. I refer to the solution of material underground and the consequent subsidence of overlying rocks. The artificial removal of salt in the New Red Sandstone areas of North-west England and the consequent subsidence of the ground is well known to all, and the removal of limestone may have given rise to some of the smaller lakes that occur in limestone regions in Britain, owing to the falling-in of the roofs of cavernous hollows underground. The Königsee has been cited by Credner as a lake which was probably formed by the removal of soluble matter from beneath it; and many other instances probably occur.

(iv.) *Lakes lying in hollows of erosion.*—The formation of rock-basins by erosion is a question which has given rise to a great deal of controversy, and the origin of rock-basins by erosion is still to a large extent enveloped in mystery. There are many lakes, apparently rock-basins, which lie in hollows so diminutive that no one would invoke the action of differential movement to account for their formation, and should their existence as true rock-basins be established they must be admitted to have been produced by erosion, though it by no means follows that the agent of erosion must necessarily have been ice. In our own country Mr. Watts has claimed the lower lakelet in the hollow of Cwm Glas in Snowdon as "certainly confined in a rock-basin, as rock occurs at its actual outlet and at every point where any former outlet might have been possible"(12). M. Delabecque, in the paper on the lakes of the Vosges district to which I have already referred, describes a lakelet of some size (L. Retournemer) which the geological map indicates as being barred by moraine, but states that to him it appears to be entirely situate within rock *in situ*, and that it must be grouped amongst the rock-basins of which the origin is still but little explained. The work of M. Delabecque is evidently marked by much care, and he has paid such special attention to lakes with particular reference to their existence in rock-basins that he is hardly likely to have overlooked any drift-filled depression to which this lakelet may be due. Mr. H. P. Cushing (13) describes diminutive lakes on the tops of low hills bordering the Muir Glacier of Alaska. Several are situated on nunataks near the end of the glacier and occupy small depressions or basins on the tops of ridges; they are only a few yards in diameter and of no great depth. "Some of them clearly occupy rock-basins, rock in place being readily traced all round them . . . other lakes have a portion of their shores formed by glacial debris. . . . That the glacier has done little more than to remove the loosened rock and polish the resulting surface is shown in a vast number of localities. . . . All these basins which I saw lie in small valleys on the mountain tops, whose presence seemed to depend on the fissure systems and on the varying depths

to which loosening of blocks had taken place." The formation of these little rock-basins, then, appears to have been primarily due to unequal weathering, the sole work of the glacier having been to remove the weathered blocks.

During the present year I had an opportunity of seeing some very tiny pools produced by unequal weathering in a way which, if conducted on a larger scale, might give rise to small lakelets. In the Langstrath Valley of the English Lake District is an alluvial plain, terminated by a gorge which I have elsewhere described as apparently due to the stream issuing from a vanished lake. A wall runs at right angles to the gorge on the east side. On the south side of the wall some exquisite potholes are seen at a considerable distance above the stream, though these are not the rock-basins to which I wish to refer. On the north side of the wall are shallow depressions in the rock, in various stages of formation. One, eighteen inches long and three inches deep, was filled with water, but it had a deposit of peaty silt on its floor. Another "basin" about three feet long was filled with heather, grass and moss, and a similar patch of moss, grass and heather occupied a space about twelve feet long, surrounded on all sides by naked rock. It is well known that the organic acids supplied by vegetation assist the weathering of rock, and that the water is also held against the rock for a much longer period where the rock is covered by vegetation than where bare, so that vegetation tends to cause the rock to be etched away beneath it, and in this Langstrath rock one saw how small lakelets might be produced, and indeed were in actual process of formation on flattish surfaces of rock owing to this cause. It is doubtful how large a basin could be produced in this way, but I see no reason why considerable lakelets should not arise in this manner after the death of the vegetation which had covered the rock. It is well known that peat-mosses are frequently due to the former existence of lakes, but it is not so generally recognised that, conversely, lakelets may be due to the former existence of peat-mosses.

I have been induced to write this paper on lakes because I believe that there is still much valuable work to be accomplished even in our country with reference to the origin of

lakes. We have lakes of many kinds—the fenland meres, the Norfolk Broads, the kettle-holes of our drift-covered lowlands, the valley lakes of upland regions and the gloomy tarns of the mountain recesses—all differing from one another in many characteristics, but broadly grouped together under the one heading, "lake". Because some of these have been accounted for, it by no means follows that the origin of all has been explained, and the student of physical geography may well be occupied for many years to come in working out problems connected with the Limnology of the British Isles.

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THE CAUSES OF VARIATION.

IN the last few years an ever-increasing amount of attention has been given to the study of the facts of variation, for it has gradually become more and more evident that this forms one of the most important vantage grounds from which the problems of evolution may be attacked. The importance of this study depends not a little on the consideration that it is based upon the collection and determination of sure and solid facts, which do not depend for their veracity upon the validity of any particular hypothesis or theoretical consideration. Even more interesting, in many ways, than the study of the *facts* of variation is that of the *causes* of variation. To what are the differences in form and structure, often by no means slight, which individuals of a species show among themselves, due? Why is it that offspring do not bear an exact resemblance to the physical form of their parents?

Into this question of the causes of variation, Darwin, who was the first to show the vital importance of the study of the facts of variation with reference to the problems of evolution, entered but little. He considered that variation "is generally related to the conditions of life to which each species has been exposed during several successive generations," and that "changed conditions act in two ways, directly on the whole organisation, or on certain parts alone, and indirectly through the reproductive system" (1). Another view which has been frequently upheld, especially by certain American scientists, is that variation is the result of an inherent tendency on the part of the organism to vary. More recently it has been shown that the problem of the causes of variation is intimately bound up with the question of sex, for the characters of offspring are produced by a mingling of those of their parents, and this mingling may not be in fixed and exactly similar proportions. Thus Wallace believes diversity of sex to be of primary importance as the cause of variation (3). Weismann also is con-

vinced that conjugation in unicellular and sexual reproduction in multicellular organisms are the means of producing variation (4). But the ultimate cause of hereditary variations he considers to be due to the constant recurrence of slight inequalities of nutrition in the germ plasm, or reproductive substance of the organism. Weismann also considers that variation depends upon the action of environment, but as he does not believe that variations so produced can be transmitted to the next generation, he is compelled to adopt the hypothesis mentioned. His views are best illustrated by a concrete example, the case of twins in the human race. As a rule, twins differ from each other in mental and physical characteristics just as much as do children of the same parents born consecutively. Occasionally, however, the twins show so close a resemblance that they cannot be distinguished from each other by strangers. These may be called "identical" twins, and the former "dissimilar" twins. In all probability dissimilar twins are derived from two ova, which have of course been fertilised by two different spermatozoa, whilst "identical" twins are derived from a single ovum and spermatozoon. On careful physical measurement, it is found that "identical" twins do show slight differences from each other in respect of the size and relation of parts. These differences are probably due to variations in the external conditions of environment during growth. The large and obvious differences of "dissimilar" twins are, on the contrary, due to differences of the germ plasm, and are determined at the time of fertilisation of the ovum.

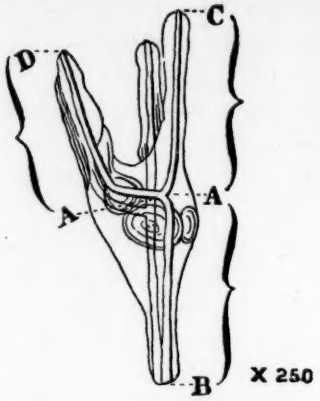
To mention still another theory as to the cause of variation, Montgomery considers that it is due to the disturbance of correlation of the organs, resulting from the change of environment, and is thus only indirectly due to change of environment (2).

All the theories which have been advanced as to the causes of variation may thus be practically divided into two classes—(i.) theories which regard them as due to some inherent tendency of the organism to vary, or to variations of the germ plasm; that is to say, theories which are

from their nature not capable of verification by experiment; and (ii.) theories which regard them as due to the action of environment, and which are therefore to some extent capable of being put to actual experimental test.

In the present paper the writer proposes to give a short account of some experiments he has recently been making for the purpose of determining the exact amount of variation which is produced in the size of an organism by definite changes in the conditions of environment. Similar experiments have of course been made before, but as a rule the changes in the environment have been made of such a kind and such a degree as do not occur in a state of nature, and, moreover, it is but seldom that the actual numerical effect upon the size of the organism has been carefully measured. In the present experiments the changes of environment were, in the majority of cases, only such as might occur under natural conditions, and the effect produced was accurately determined by measuring large numbers of the organisms allowed to develop under the varying conditions, and calculating mean values. For such experiments as these, it is essential to work with an organism that can be readily obtained in large numbers, and which can be easily kept under artificial conditions. The larvæ or plutei of sea-urchins answer to such requirements most admirably, and they were in fact the organisms chosen. Thus these larvæ can always be obtained in innumerable quantities by the simple process of artificially fertilising ova obtained by shaking pieces of the ovaries of the sea-urchins in water, by adding other water containing spermatozoa obtained in a similar manner by shaking pieces of the testes. In a day or two these fertilised ova, which are transferred to glass jars holding 2 or $3\frac{1}{2}$ litres of water, develop into free swimming larvæ. These continue to grow in size up to a certain point. In order to measure their size, they are killed by adding ·25 per cent. of corrosive sublimate to the water, preserving them in alcohol and, after mounting them in glycerine, measuring them under the microscope with a micrometer eyepiece. These larvæ are peculiarly easy to

measure as they contain a very sharply defined calcareous skeleton, of the form shown in the figure.



In every case the length of the body AB, and the lengths of the oral and aboral arms AD and AC, were measured. In order to diminish the error of observation as much as possible, fifty larvæ were measured in each case and a mean taken; also, as a rule, several sets of observations were made in respect of each environmental condition investigated. This necessitated a very large number of measurements, so that altogether some ten thousand larvæ were measured. The larvæ were, as a rule, allowed to develop for eight days before measuring, because the arm lengths reach their maximum length in this period. After this they gradually undergo absorption.

The environmental condition investigated most thoroughly was that of temperature, because in the first place this is one which undergoes such frequent and considerable variations under natural conditions, and also some interesting and unexpected results were obtained with regard to it, which necessitated more extended inquiry. These unexpected results relate to the temperature of the water at the time of impregnation of the ova. It might be reasonably expected that the temperature would at this moment have no more influence upon the subsequent size of the larvæ than it has at any other time; but such is not the case. So sensitive are the ova and spermatozoa to tem-

perature at the time of impregnation that a difference of a few degrees of warmth or cold lasting for only a minute at this time has as great an influence as a similar difference extending over the whole eight days of subsequent development. To come to figures, it was found that as a mean of eight sets of observations, ova impregnated at about 8° C. were 4·2 per cent. smaller than those impregnated at from 17·6° to 21·6°, the subsequent conditions of development being identical. Also, as a mean of five observations, those impregnated at about 25° C. were 5·2 per cent. smaller. In all these observations the ova were kept in the cold or warmed water for an hour at the time of impregnation, but in seven other experiments they were kept for only a minute at 8° or 25°, and were then transferred to water at a normal temperature. Nevertheless the larvæ were, after eight days' growth, on an average 4·1 per cent. smaller. In four instances the ova were kept for only ten seconds during impregnation at the abnormal temperature, the larvæ being then on an average only 1·7 per cent. smaller. We may presume that the time was so short that less than half the ova were in these cases impregnated at the abnormal temperature.

In addition to the observations cited, about thirty other sets of determinations on the effect of temperature at the time of impregnation were made, some of them at temperatures, as 15° and 23·5°, intermediate between the extremes mentioned above, and others of them in which the larvæ were measured after four, five or six days' development. They all serve to confirm the fact of the special sensitiveness of the ova to temperature at the time of impregnation.

Experiments were also made on the effect of temperature during the course of development. Larvæ kept at 23° to 24° for the whole period of eight days were only about 2 per cent. smaller than those kept at 20°; were, in fact, affected to just about the same extent as when the temperature of impregnation was 23° to 24°. Also larvæ kept at about 15° during development were about 4 per cent. smaller than others kept at 19°.

It is one of the chief recommendations for the use of sea-urchin larvæ in experiments on variation that artificial fertilisations can be carried out all the year round. In a state of nature probably these organisms only breed for a few months in the winter and early spring, but at Naples, for these experiments were carried out at the Zoological Station situated there, I was able to obtain larvæ through all the summer months. It was interesting to note, however, that larvæ obtained in these months were much smaller than those obtained in the spring, those in August being veritable dwarfs, some 20 per cent. or more smaller than those obtained in April, May and October. Those in June and July were intermediate in size. This dwarfing of the larvæ is presumably due to the comparative immaturity of the ova and spermatozoa in the off-breeding season, for the conditions of development were in other respects practically the same. It suggests a reflection as to the possibility of a somewhat similar condition existing amongst higher animals, *i.e.*, as to whether the offspring of animals conceived at the beginning or end of the term of reproductive capacity are any smaller, or less well formed than those arising in the plenitude of functional power. At present there appears to be no evidence that this is the case, though this may be due to its not having been sought for.

The next condition of environment to be examined was that of the salinity of the sea water. The larvæ were found to be exceedingly sensitive to changes in this condition. Thus larvæ allowed to develop in water to which one part in twenty of fresh water had been added, were 15·6 per cent. larger than the normal, and in that containing one part in forty of fresh water, 9·5 per cent. larger. The addition of too much fresh water has a harmful effect however, as with one part in seven of fresh water, the larvæ were 4·3 per cent. smaller. By more concentrated water than the normal the larvæ were apparently practically unaffected. This striking reaction to slight changes in the salinity of the sea water must frequently be in operation under natural conditions. Thus in the *Challenger* Expedition it was

found that, for instance, between Bermuda and New York the specific gravity of the sea water varied from 1·02504 to 1·02723 at 15·6°, or enough to cause a difference of 13 per cent. in the size of larvæ developed under the extreme conditions. The salinity of the waters of the Bay of Naples, especially near the coast, probably shows just as great if not greater variations than these.

As the salinity of the water, or percentage of inorganic salts present, has such a marked influence on the growth of the larvæ, it might be expected that they would react similarly to changes in the organic constituents present. Such proved to be the case. Thus larvæ allowed to develop in water in which other larvæ had already undergone development, and which therefore contained their products of excretion, were diminished in size some 7·5 per. cent. It was thought that this harmful effect might be partly due to the presence of uric acid and urea excreted by the larvæ, but such was evidently not the case, as the addition of small quantities of either of these salts to the water provoked a considerable increase in the size of the larvæ. Thus by the introduction of one part in 70,000 of uric acid, larvæ 12·2 per cent. larger than the normal were produced. With larger quantities of uric acid the effect produced was less, till with one part in 28,000, larvæ 2·1 per cent. smaller than the normal resulted. In water containing about one in 60,000 of urea, the larvæ were found to be about 3 per cent. larger than those grown under normal conditions. This result is very curious, and was quite unexpected. No less curious are the results of the experiments made on the effects of changes in the gases dissolved in the water. Thus it was found that the amount of oxygen gas dissolved in the water might be diminished considerably without producing any effect, and, strange to say, that the amount of carbonic acid present might be increased by some 18 per cent.; to such a degree in fact that the larvæ would barely develop at all, and yet no harmful effect on the size of the surviving larvæ show itself.

It has perhaps been noticed that no reference has been made to experiments on the effects of nutrition, this being

the environmental condition which in previous researches has generally been examined most completely, and which has been found of such powerful influence. The reason of this is twofold. In the first place, nothing is known as to the natural food of these larvæ; and in the second place, they seem to be able to develop equally well whether food is present or absent. In all probability they feed on the other minute organisms with which the sea water teems. If this is so, and if in the early stages of development supplies of food are of importance to growth, it follows that when a small number of larvæ are allowed to develop in a given volume of water they ought to grow faster than when there is a large number, for then there is only a much smaller proportionate amount of food material available. Experiment proved that within certain limits the size of the larvæ is not influenced by the number developing together. In every experiment the number of larvæ present was estimated by counting under the microscope those present in a known volume of water, and it was found, as a mean of thirty experiments in which there were on an average 4000 larvæ per litre present, the average size was almost exactly the same in thirty other experiments in which the number of larvæ was about 10,000 per litre. It was in fact found that the larvæ were undiminished in size if they were kept under about 30,000 per litre. It must be concluded therefore that in the first few days of their development these larvæ have enough food material stored in their bodies for their immediate wants.

In all the observations thus far described reference has been made only to the body lengths of the larvæ. As has been mentioned, the arm lengths were measured in addition. Hence it is possible to institute a comparison as to the effects produced in both cases. These effects, contrary to what one would suppose, run on by no means parallel lines. Thus, as we have just seen, the body lengths are practically unaffected by the number of larvæ developing in a given volume of water. The arm lengths react most markedly, so that with 4000 larvæ per litre the aboral and oral arm lengths are respectively 13·4 and 15·9 per cent. shorter than

with 500 per litre; and with 17,500 per litre they are respectively 25·9 and 23·3 per cent. shorter. Variations in the temperature of the water during development also produce opposite effects in the two cases. As we have already seen, the body lengths of larvæ allowed to develop at temperatures above 22° are about 2 per cent. shorter than of those at about 20°, but the aboral and oral arms are respectively 10·8 per cent. and 8·5 per cent. *longer*. Again, whilst the body length of larvæ grown in diluted water is on an average increased by 9·1 per cent., the arm lengths are *decreased* by 7·7 and 10·5 per cent.; or, as the arm length values are calculated as percentages on the body lengths, they are practically not affected at all. Even the arm lengths are not affected to the same extent by the same change of environmental condition. Thus the ratio between the arm lengths of larvæ grown at temperatures above 22° is 4·3 per cent. higher than of those at temperatures below 18°.

It is unnecessary to make remark upon the experiments made in respect of other conditions of environment, but a passing notice may be given to the observations which showed that the variability or amount of variation in the size of the larvæ may be affected by changes in the environment, as well as their actual average size. Thus the dwarfed larvæ obtained in the summer months varied in size some 10 per cent. more than those obtained in the spring. Again, larvæ allowed to develop at 19° varied 18 per cent. more than those allowed to develop at 17°, and 10 per cent. more than those at 23°. It would seem therefore that the variation in an organism, whether it be in degree or in the average size, with reference to a single organ or to the correlation of organs, is equally capable of being affected by changes in the conditions of environment.

This conclusion is fully supported by the results of the measurements made by Weldon upon shrimps and crabs (5). By measuring some hundreds of individuals, and taking mean values, Weldon showed that the specimens obtained from various sources did not by any means correspond in respect of the size and relation of their parts, but that they

may be looked upon as so many local races. Thus, for instance, whilst shrimps obtained near Plymouth had a mean carapace length of 250·05, those from Southport had one of 248·50, and those from Sheerness one of 247·51. The degree of variability also differed in each case, the "probable errors" being respectively 4·53, 3·17 and 3·05. Also, different organs varied to different extents. For instance, the post-spinous portion of carapace reached the maximum length in Sheerness shrimps and the minimum in those from Plymouth, whilst as we have just seen the carapace lengths varied in the opposite direction. Still again, the correlation between the various organs also varied in the different local races, though this variation was small and might perhaps have been due to experimental error.

A few remarks may be made as to the bearing of the results obtained in these experiments on the theory of Natural Selection. It has been shown that a change in the salinity of the water in which the larvæ are developing may produce a variation of 15 per cent. in their size. Now these individual larvæ are found to vary on an average by about 6 per cent. from the mean size of the whole number. If, therefore, half of a number of larvæ developing in any region were exposed to water of one degree of salinity and the other half to water of the other degree, the variability of the whole group of larvæ would be more than trebled. Other changes in the environment would produce similar, though perhaps smaller, changes. Now, other conditions equal, the more variable an organism, the greater opportunity has Natural Selection of stepping in and, by preserving the most favourable individuals, modifying the species. But, it may be objected, this is not actually the case, because the variations thus produced are, in Weismann's terminology, merely somatogenic and not blastogenic. They are merely variations in the body tissues, and, the germ plasm not being affected, are not transmissible by heredity. A little reflection will show that this objection, though in some respects valid, yet is by no means fatal to our proposition. Thus, suppose that for instance in sea-

urchin larvæ there was a tendency for the larger individuals to be preserved by the action of Natural Selection. When the individuals vary on an average by only 6 per cent. they are all so similar in size that the advantage the larger individuals have over the smaller in the struggle for existence is only very slight, and hence Natural Selection can only act very slowly and haltingly. Suppose, on the other hand, that by the action of varying conditions of environment the larvæ vary on an average by 20 per cent., then the larger individuals will have a very much greater advantage over the smaller ones than before, and will consequently be preserved in much greater proportionate numbers. Now these larvæ, though caused by the action of a variable environment to assume a considerably larger size than they would have had under a more constant one, would nevertheless under this more constant condition have still been of larger size than the other individuals, and hence will transmit to the next generation that characteristic of increased size, though to a smaller degree. Thus though the selected larvæ might be 20 per cent. larger than the average, they only contain a similar germ-plasm to larvæ 6 per cent. larger than the average when grown under a more constant environment. Hence their offspring will only be some 6 per cent. larger than those of the previous generation.

We now see the importance of the fact established in the earlier part of this paper, *viz.*, that the ova are more sensitive to changes of environment, such as temperature, at the time of impregnation than subsequently. Thus, whilst the temperature of the water in which these larvæ develop under natural conditions may show but slight variations on an average in periods extending over several weeks, yet the extreme variations within such a period may be considerable. As, therefore, ova would probably happen to undergo impregnation at all times in the breeding season, whatever the temperature of the water, the larvæ developed therefrom would have a greater variability than if the ova had been no more sensitive to temperature at one time than at another. Though it has not been conclusively proved that the ovum is specially sensitive at the time of impregnation

to other conditions than that of temperature, yet the probability is that such is the case, and that conditions such as the salinity of the water also have a more powerful influence.

Another conclusion arrived at, which is of some importance in the study of the problems of variation, is that changes of environment may produce different and opposite effects upon different parts of the same organism. Thus a fall in the temperature or a decrease in the salinity of the water produces a decrease in the arm lengths of the larvæ and an increase in the body length. It is thus possible for parts of an organism to become modified, though they may be entirely unacted upon by the process of Natural Selection, or may even serve some useful purpose to the animal. If, for instance, it is of greater utility to the larvæ that their body lengths should increase than that their arm lengths should increase, and if by the action of a fall in the temperature of the water larvæ with greater body lengths and smaller arm lengths are produced, then, on an average, the larvæ exhibiting these changed characteristics to the most marked extent will survive, and the race will be modified in this direction.

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